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# PREFACE

In 1947 the Directors of *Annual Reviews, Inc.*, asked a large number of men prominent in psychology to comment on the desirability of publishing an annual review of contemporary psychological literature. On the basis of almost unanimously favorable replies, the Directors decided in September, 1947 to publish an *Annual Review of Psychology*, beginning early in 1950.

Among psychologists acquainted with the immediate success of the *Annual Review of Physiology*, begun in 1939, there was some preliminary consideration of the feasibility of undertaking a comparable volume in psychology during the early forties. Unfortunately, the outbreak of World War II soon made it apparent that reviewers would be unable to survey world literature in the field of psychology. Renewed interest in and discussion of the project after the war, however, led to the present plan of publication, which is to present critical appraisals of current research and theory in psychology on an annual basis in the case of the most active and general fields, and on a biennial basis for fields of lesser activity or scope.

The *Annual Review of Psychology* was conceived as a supplement to, rather than a duplication of, other publications presenting abstracts or relatively long-term reviews of psychological literature. To carry out the proposed program, *Annual Reviews, Inc.*, organized an editorial board consisting of an Editor, an Associate Editor, and an Editorial Committee composed of five psychologists representing in interest and familiarity as many special fields of psychology as possible.

Eighteen fields were selected for review in Volume I. Although leaving to each author the choice of material, the Editorial Committee deemed it desirable to specify that reviews cover principally publications during the previous year which the authors considered most noteworthy. They were at liberty to indicate their points of departure by citing summaries of basic work prior to the review period but were advised not to strive for mere comprehensiveness, especially in the form of a loosely integrated series of abstracts of all of the current literature. On the contrary, the editorial board asked that they adopt an interpretative and evaluative approach to the literature they selected for review. While it appears that all fields are not equally amenable to this approach, it is hoped that future volumes of the *Review* will reflect gains in the techniques of exposition and interpretation of the desired kind. Since the boundaries of fields for review which the editorial board has selected are subject to considerable controversy, future publications will undoubtedly reflect further compromise and modification in certain of the subdivisions which for this volume were allocated on a trial basis.

Two factors—the unsettled state of many centers of learning and the desire to prepare the reviews for early publication—produced certain shortcomings in the present volume which the editorial board will attempt to remedy in future volumes. From the outset, the editorial board intended that literature from all laboratories of psychology in the world would be reviewed. The continuance of limited circulation of scientific journals, in part, prevented the realization of this intention. In some instances, also, scholars outside the United States to whom early inquiries as to their availability for reviews were sent found themselves unable to undertake the project within the comparatively short notice of one year. In later volumes it is hoped that many scholars outside the United States will be willing and able to review their fields of specialization and thus insure the reader a world-wide survey of recent literature in psychology and ancillary subjects.

The factor of time further operated to produce a certain amount of overlapping in the reviews. The Editors, lacking time to obtain the approval of contributors for selective deletions of duplicate material, have permitted a considerable amount of overlapping material to remain in the present volume. The inevitable overlap of interest in certain fields having ill-defined boundaries will always require a certain amount of duplication; however, it is expected that many readers, whose attention will naturally be centered on particular chapters rather than the entire volume, will not be inconvenienced by the presence of similar material elsewhere.

The reader will also note a departure from methods of citation now well established in the majority of psychological journals. In the interests of economy and in accord with the policies of other publications of *Annual Reviews, Inc.*, references do not include titles of the articles cited. Also, in some instances citations are not given for well-known works referred to for the purpose of introducing specific papers or a general topic. It is hoped, however, that the references are sufficiently numerous and complete to permit the reader to locate any material he desires to examine further.

Finally, the Editors recognize that not all readers will be equally well served by this attempt of the *Annual Review of Psychology* to present succinct epitomes of important contributions in contemporary psychology. Although the researcher will find the *Review* helpful by directing his attention to publications which other specialists deem significant, he may still be forced to consult the original publications for additional details.

Possibly the *Annual Review* will be most useful to graduate students and to teachers who, because of the profusion of current literature, will welcome an annual, single-volume digest and evaluation of current literature in their fields of interest. In addition the *Review* should supplement and extend standard textbooks which, though still useful in other respects, have fallen behind the current investigations. Lastly, the Editors hope that the *Review* will be of advantage to those who are not themselves

psychologists, but who wish to follow current developments in the field without concentrated attention on the constant flow of new publication. If it achieves this objective, the *Annual Review of Psychology* will become a reliable guide to psychological literature for sociologists, physiologists, educators, personnel specialists, social workers, nurses, and psychiatrists.

J.E.A. C.I.H.
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# TOPICS AND AUTHORS

# ANNUAL REVIEW OF PSYCHOLOGY

# VOLUME II (1951)


CHILD PSYCHOLOGY, *R. G. Barker*

LEARNING, *C. E. Buxton*

VISION, *A. Chapanis*

AUDITION, *E. G. Wever*

SOMESTHESIS AND THE CHEMICAL SENSES, *C. Pfaffman*

INDIVIDUAL DIFFERENCES, *L. E. Tyler*

PERSONALITY, *D. W. MacKinnon*

SOCIAL AND GROUP PROCESSES, *D. Katz*

INDUSTRIAL PSYCHOLOGY, *R. M. Bellows*

COMPARATIVE AND PHYSIOLOGICAL PSYCHOLOGY, *C. T. Morgan and J. E. Deese*

ABNORMALITIES OF BEHAVIOR, *W. Overholser*

CLINICAL METHODS: DIAGNOSTICS, *R. C. Challman*

CLINICAL METHODS: PSYCHOTHERAPY, *S. R. Hathaway*

EDUCATIONAL PSYCHOLOGY, *J. B. Stroud*

COUNSELING METHODS: DIAGNOSTICS, *D. B. Stuit*

COUNSELING METHODS: THERAPY. *H. B. Pepinsky*

STATISTICAL THEORY AND RESEARCH DESIGN, *A. L. Edwards*

GERONTOLOGY (LATER MATURITY), *N. W. Shock*


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# CONTENTS


GROWTH, DEVELOPMENT, AND DECLINE, *H. E. Jones and N. Bayley*. 1

LEARNING, *A. W. Melton* . . . . . 9

VISION, *N. R. Bartlett* . . . . . 31

HEARING, *E. B. Newman* . . . . . 49

SOMETHESIS AND THE CHEMICAL SENSES, *F. A. Geldard* . . . 71

INDIVIDUAL DIFFERENCES, *R. L. Thorndike* . . . . . 87

PERSONALITY, *R. R. Sears* . . . . . 105

SOCIAL PSYCHOLOGY AND GROUP PROCESSES, *J. S. Bruner* . . . 119

INDUSTRIAL PSYCHOLOGY, *C. L. Shartle* . . . . . 151

ANIMAL AND PHYSIOLOGICAL PSYCHOLOGY, *D. O. Hebb* . . . . 173

ABNORMALITIES OF BEHAVIOR, *N. A. Cameron* . . . . . 189

CLINICAL METHODS: PSYCHODIAGNOSTICS, *H. F. Hunt* . . . . 207

CLINICAL METHODS: PSYCHOTHERAPY, *W. U. Snyder* . . . . 221

EDUCATIONAL PSYCHOLOGY, *L. J. Cronbach* . . . . . 235

COUNSELING METHODS: DIAGNOSTICS, *R. F. Berdie* . . . . . 255

COUNSELING METHODS: THERAPY, *E. S. Bordin* . . . . . 267

STATISTICAL THEORY AND RESEARCH DESIGN, *D. A. Grant* . . 277

PROBLEM SOLVING AND SYMBOLIC PROCESSES, *D. M. Johnson* . . 297

INDEXES . . . . . 311




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# GROWTH, DEVELOPMENT, AND DECLINE[1]

By Harold E. Jones and Nancy Bayley
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Current investigations of the development of motor and mental functions are with only a few exceptions based on cross-sectional data, and hence are limited to normative statements or to mass correlational studies of relationships. An often announced goal of developmental psychology is the analysis of growth processes and the portrayal of individual development through the use of repeated tests and observations of the same subjects. Except in a few research centers, however, psychologists are as yet making little systematic use of the longitudinal approach.

## Motor Functions

In the reports included in this review the earliest behavior level investigated is that of the seven-month fetus. Spelt (1) describes an experiment in which a conditioned response was established to a loud noise in the human fetus during the last two months of gestation. The sample included 16 cases, some of which were controls. Three cases in which conditioning was definitely established are reported in detail.

The development of speech sounds in infants from birth to two and a half years is analyzed by Irwin in a series of four articles (2 to 5). In the first two months of life only three of the vowel sounds are used, but by two and a half years children employ most of the sounds in adult speech, and in almost the same frequency. Sounds made at the front, back, and middle of the mouth change in relative frequency during the first two years; the frontal vowels are predominant at all ages, but less so in the later months. New sound types and sound frequencies increase rapidly with age. The rate of growth in these vocal functions appears to be independent of the presence or absence of siblings, but is correlated with socioeconomic status.

In a series of careful observations of thumb-sucking among 143 infants in an orphanage, between birth and one year, Kunst (6) noted thumb- or finger-sucking at some time in almost every infant. Occurring in a few cases soon after birth, the incidence increased rapidly to three months, with a gradual decrease between six and ten months, and an apparent secondary increase toward the end of the first year. In this study the frequency was greater among boys.

The method of cinemanalysis, which has been used so effectively by Gesell and his associates in studying various aspects of behavior in infancy and early childhood, was applied by Ames (7) in the study of developmental trends in manual orientation and postural adjustment, in connec-

[1] This review covers the period from January, 1948 to July, 1949.

tion with writing and block building. Ames was not concerned with normative measures of the efficiency of performance, but with the question of whether basic orientational patterns could be defined and be shown to vary in the course of development. From nine months to ten years sequential changes were observed in writing, involving both the preferred place of writing and the position of the minor hand. Developmental factors were also shown in body orientation and in the direction of placement in block building.

Manual performances of two-, three-, and four-year-old children were analyzed by Hildreth (8) with reference to right hand dominance. Results from tests and observations indicated a trend toward increasing use of the right hand. Acts most subject to training (eating with silverware, drawing, etc.) were more likely to involve a stereotyped use of the right hand. Although the degree of dextrality is apparently related to training and use, it is not implied that the initial tendency toward right handedness is of environmental origin.

Developmental changes in physical abilities are discussed in two reports. A book by Jones (9) in the University of California Series in Child Development describes the development of four aspects of static dynamometric strength during adolescence, in relation to various physical and physiological measures. Cumulative records were obtained at six-month intervals from age 11 to age 17.5, on a public school sample of 176 cases. Growth curves for boys and girls were approximately parallel to age 13, but diverged sharply during the next four years, due to accelerated growth of boys and decelerating growth of girls. Sex differences were also noted in the rhythm of growth in relation to season of the year (more rapid growth in the spring, particularly in girls), and in the degree of dextrality (girls averaged about 12 per cent stronger in the right than in the left hand, boys about 6 per cent stronger). Markedly different average growth curves were shown for the early- and late-maturing, and for boys high or low in the mesomorphic component of body build.

The relationship of strength to physical maturity was also examined by Nevers (10) in a cross-sectional sample of 235 boys. Nevers classified his cases according to Crampton's criteria of prepubescent, pubescent and postpubescent. These groups showed different levels of performance in strength and also in track scores. Within each group, strength was positively correlated with McCloy's Classification Index, which combines age, height, and weight.

Reaction time (hand and body) was studied by Atwell & Elbel (11) on a sample of 247 boys, ages 14 to 17. It is well-known that in reaction time tests the quickness of response increases most rapidly during the first decade of life; in an earlier study Jones (37) has demonstrated small gains in the finger reaction time to sound in boys from 11 to 14 years of age, with a plateau beginning at about 14. In the reactions represented in the present study, small gains apparently continue into later adolescence.

"Adolescent hoarseness" and the voice change in adolescence were

investigated by Curry (12). The literature on this subject shows considerable disagreement as to the nature and origin of hoarseness, and even as to the existence of developmental changes in this characteristic. From age 10 to 14, Curry found a marked increase in the proportion of subjects with voices diagnosed as "hoarse and husky."

### Mental Abilities

The problem of the consistency of mental growth, and of the prediction of later status from tests given in infancy, is still subject to controversy. Gilliland (13) has prepared a new scale of 40 items applicable to infants from birth to three months, the Northwestern Infant Intelligence Test, which he regards as giving promise of greater predictive efficiency than is usually reported. The relationship to later attainment has not yet been ascertained.

The stability of mental test performance at 14 ages between 2 and 18 years was investigated by Honzik, Macfarlane & Allen (14) for 252 children from a representative sample of families in Berkeley, California. As in previous studies, they found that constancy is closely related to the age period of the testing and to the interval between tests, with greater stability at the later ages, and over shorter intervals. Between 6 and 18 years almost 60 per cent of the group changed 15 or more IQ points, while 9 per cent changed 30 or more points. Studies of individuals showed consistent trends in IQ over long periods, often in the direction of the family level of abilities. Children who manifest unstable variation in IQ tend to show corresponding variations in emotional stresses and strains. Such IQ variations do not occur, however, in all cases of emotional disturbance.

In a further study of children in adoptive homes, Skeels & Harms (15) have reported on the later intelligence levels (median age six years) of children placed in foster homes prior to the age of six months. In one group of 87 cases all mothers had IQ's of 75 or less; the IQ range of the children was from 55 to 141, correlating 0.23 with the true mother's IQ. From the data of this and other groups, the writers conclude that "children of mothers with low intelligence, or from fathers with low occupational status, or from a combination of both, placed in adoptive homes in infancy, attain a mental level which equals or exceeds that of the population as a whole." Almost half of the tests on which this conclusion is based were administered in the preschool ages, including an unspecified number between one and two years of age.

Socioeconomic differences in mental growth, as measured by scores on our conventional intelligence tests, have been analyzed by Davis (16) with reference to the role of class differences in experiences, values, and test motivation. The attempt has frequently been made to interpret social class differences in mental test scores solely in terms of psychometric factors (inappropriateness of testing procedures) or in terms of growth handicaps among underprivileged children. It seems premature, however, on the basis of the evidence presented, to conclude that selective factors

(with a genetic basis) are wholly inoperative in determining differences among social groups.

The effect of poliomyelitis upon mental development was investigated by Phillips, Berman & Hanson (17) in a well-controlled study. One hundred and one children who had poliomyelitis in the 1946 epidemic and whose early Stanford-Binet records were on file in the Minneapolis public schools were matched with controls on the basis of intelligence, chronological age, sex, school, and grade. The two samples were similar in socioeconomic status. Retests of both groups in 1947 showed a significant difference in favor of the control group, in Stanford-Binet IQ and on the Porteus mazes. The difference was especially marked among the younger children and among boys. An intensive study of 22 cases with an average IQ loss of 19 points indicated that in these children the illness had been relatively severe. The origin of these differences in mental test scores is difficult to define. The majority of earlier studies have failed to show a significant degree of retardation. When retardation does occur among the survivors of poliomyelitis, it is difficult to rule out the effects of absence from school, and of various social and psychological sequelae of illness. Even though temporary, however, the apparent lag in mental development may be of some importance in relation to the individual's total adjustment. In the present study, no differences were found between the controls and the children who had been ill, in scores from the Hunt-Minnesota test of organic brain damage.

The "Lima Revision" of the Terman group test was applied by Blumenfeld (18) to a large sample of Peruvian school children. Results are presented by grade level but not by age. Lima norms are lower than the Terman norms, and in this sample the average scores for boys were significantly higher and somewhat more variable than scores for girls.

Analyzing sets of two or more retests for 1,000 individuals on the American Council Examination, Thorndike (19) reported additional evidence for gains in scores in late adolescence. Annual gains became smaller in amount after about 16 years of age, reaching a point of zero gain at 21.5 years (extrapolated by a quadratic solution) or at 25.75 years (linear extrapolation).

The adult mental level of Terman's group of "gifted children" was evaluated by Thorndike (38), through an ingenious series of comparisons using as a basis the general adult norms on a vocabulary test, for a Gallup poll sample. The Terman group falls slightly above the 95th percentile of the Gallup sample. This was interpreted as implying a regression from their childhood score, from about 3 $\sigma$'s to about 1.7 $\sigma$'s above the mean. Regression would be less, if the Gallup sample is actually more selected than the original Stanford-Binet standardization group.

Foulds (20) and Foulds & Raven (21) have investigated age changes in test intelligence through 60 years, using two tests standardized in England (the Mill Hill Vocabulary Test, and the nonverbal Progressive Matrices, 1938 test). Their studies show the expected differences in mean

scores for different occupational groups of adult males. Age trends are similar, however, for all occupation groups and intelligence levels. Nonverbal test scores show a peak at the age of about 16 years, with fairly rapid and continued decline after from 20 to 25 years. Vocabulary scores, on the other hand, are highest at the age of 30 years and remain at a fairly high level through 60 years, except for moderate declines in those making below average scores. A later form of the Progressive Matrices, 1947 test, is described by Foulds (20) as possessing adequate range for testing superior abilities. The test scores of university students, on this form, were normally distributed.

A quantitative study of vocabulary in later maturity was made by Fox & Birren (22) with a sample of 216 native whites in a home for older persons. Their average age was 71 (range, from 40 to 99), mean education six years, and median period of institutionalization three years. The Wechsler-Bellevue and Seashore-Eckerson vocabulary tests were used. As in other studies, a positive correlation was found between vocabulary and years of education ($r$'s of 0.38 and 0.34), but in this sample there was no relation between size of vocabulary and age, length of institutionalization, auditory defects, or visual defects. The failure to obtain a relationship to age is not a new finding, but in this and similar studies it may be conjectured that individuals institutionalized at a later age may in some respects constitute a superior sample. The difficulty of making adequate allowance for such factors in a cross-sectional study again emphasizes the need for the more extensive use of longitudinal methods.

Greater evidence of vocabulary changes may be obtained when the quality of responses rather than merely the apparent size of vocabulary is considered. Feifel (23) analyzed the responses of 370 normal and abnormal men and women, aged 15 to 80 years, on the Revised Stanford-Binet vocabulary test. When they were matched for education, age, and vocabulary score, the abnormals were significantly poorer than normals in the quality of responses and became increasingly poorer with advancing age. Answers were more discrete, concrete, and descriptive, and less generalization was found than among the normals. These regressive changes in the quality of response imply a degree of language deterioration greater than would be expected from quantitative vocabulary studies. As several writers have suggested, ordinary vocabulary scores may fail to reflect deterioration because of the relatively easy passing standards and the possibility of acceptable alternative responses of poorer quality. Feifel also found some indications of qualitative change among the older normal subjects, but the regression was much less extensive than among abnormals.

Kallmann & Sander (24) give a first report of an extensive study of 1,602 twins, aged 60 years and older. They found remarkable similarity in monozygotic, as contrasted with dizygotic twins, in longevity and in various physical and mental traits. Intelligence and personality measures of the monozygotic twins remained similar even though members of a

pair had lived for many years under very different environmental conditions. Senescent changes, both mental and physical, were interpreted as pointing to the strong influence of genetic factors.

Literary output in relation to age was studied by Lehman & Heidler (25), on the basis of evaluations by 50 literary historians and critics. Definite age changes were found both in quality and quantity of literary performance, with quality tending to fall off earlier than quantity. The youngest age at the time of greatest production for poetry was found to be from 25 to 29 years; comedies and tragedies, from 33 to 37 years; and novels, from 40 to 44 years. In individual cases, the best literary output may be found at any age, from before 20 to after 70 years.

Mental test performance over the age range of from 17 to 62 years is reported by Lewinski (26) for 1,000 men in the U.S. Navy who were tested with the Wechsler Adult Intelligence Scale. Both verbal and performance scores tended to increase slightly with age. The positive correlation of 0.13 ($\sigma_r$ is 0.03) between the total scale and chronological age is surprising in view of the negative correlations usually obtained in this age range. Contrary to the writer's assumption of a random sample of United States white males, it seems probable that the test group was more highly selected at the upper ages. Educational attainment was shown to correlate 0.61 with the verbal scale and 0.44 with the performance scale, but no evidence is given as to educational backgrounds at the various age levels.

The relation of age and the developmental processes to changes in mental organization is discussed by Hsü (27) with reference to results from various factor analysis studies of mental traits. He points out that the studies from which Spearman postulated a general intelligence factor were based on test scores made by children, while Thurstone's multiple factors were derived from tests of young adults. From these and similar studies, he develops the hypothesis that in infancy there is one general factor, but that with development there is a "differential bio-process through which the original potentialities are differentiated in their course from undifferentiated patterns to definitely clear-cut configurations." Hsü's position seems to be in conflict with the empirical findings of Chen & Chow (28), who made a multiple factor analysis of a test battery applied at three age levels (grade 4, senior high, and high school graduates) in a relatively homogeneous Chinese population. Evidence was found for a general factor in all the groups tested, and for an increase in this factor with age, up to the college level. The tendency for factor patterns to become simpler, with advancing developmental level, was interpreted as a tendency to integration in mental development.

### Other Developmental Functions

Synolds & Pronko (29), using the Dvorine Color Perception charts, found that the percentage of correct naming responses ranged from zero at age three to 25 per cent at age four and above 90 per cent after age

seven. Marked improvement occurred between the fifth and sixth years.

The ability to perceive change of color in painting was tested by Webber & Hunnicutt (30), using three drawings with outlined areas of color change. Among approximately 400 children, grades 1 to 9, correct responses increased from 14 per cent at five years to 43 per cent at age 14. Low correlations were found with mental age.

In a study of color vision, Boice, Tinker & Paterson (31) report a significantly greater incidence of color blindness in men over 60 years of age. Decreasing acuity with advancing age, in this and other senses, could be expected to contribute to progressively less adequate sensory-motor functioning.

The development of space concepts was studied by Ames & Learned (32) through the use of questions ("Where do you live?", etc.) and by the recording of spontaneous verbalizations. Normative data for six age levels, from 18 to 42 months, show a spurt at around 30 months in the use of space terms.

Children's use of indeterminate number concepts ("several," "many," etc.) was studied by Brotherton, Read & Pratt (33) in a comparison of three age levels, grades 2 to 11. Differences are reported for specific terms (e.g., younger children prefer the use of "some"; older children, "few" and "several"), but no generalizations could be made as to common trends.

Learning ability as a function of age was studied by Hunter & Bartlett (34), who tested 31 children from two to seven years of age, on a double alternation problem. A marked change in ability to solve the problem occurred at the three-year level. An important factor in rapid learning was a "problem-solving attitude," rather than merely an interest in gaining the reward.

A test of musical ability and appreciation was developed by Wing (35). The test scores increased with age to 16 years, after which they remained stable. Age changes in scores were at least in part independent of continued training in music.

The relation of articulatory handicaps to language development was examined by Yedinack (36) in a study of second grade children with functional articulation defects. As compared with controls, they were significantly inferior in oral and silent reading and showed a tendency to poorer vocabulary development. Data were lacking to determine to what extent this relationship might be due to common causes, or to the influence of one handicap upon another.

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# LEARNING[1]

By Arthur W. Melton

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The psychology of learning has been vigorously pursued, both in experiment and theory, during the past year although the quantity of publications in this field may be proportionally on the decrease (1). For example, in the 10 issues of the *Psychological Review* up through July, 1949 there were 48 articles of which 17 were concerned explicitly with some aspect of the learning process. Similarly, in the nine issues of the *Journal of Experimental Psychology* there were 114 major articles of which 62 had to do with some aspect of learning or retention. This vigor, although determined by the energies of a relatively small proportion of all psychologists, reflects a widely held conviction that the psychology of learning is central in any adequate theory of behavior or experience. It is the science of the acquisition and utilization of perceptual, motor, intellectual, affective, and motivational habits, and as such infiltrates the discussion of these phenomena as well as the concepts of individual aptitudes and abilities.

These past 20 years of experimental-theoretical development have been increasingly under the influence of the opposed theoretical systems of Tolman (2) and Hull (3), and a presentation of the principal advances since mid-1948 will reveal this. On the other hand, there has been a continuation of the Thorndikian (4, 5) approach, with its emphasis on experiments with human subjects and with symbolic rewards and punishments, and a continuation of the functionalist approach to the problems of transfer of learning and retention. The present review will, therefore, be organized in terms of the major theoretical problems of the learning process, including transfer of learning and retention, but with emphasis on the theoretical differences of the Tolman and Hull groups. No attempt is made to cite all the experimental literature of the period.

## Current Status of Theories of Learning

The multiplicity of contemporary theories of learning, and yet the feasibility of dichotomy into "stimulus-response" (S-R) and "field" theories, is clearly revealed in Hilgard's *Theories of Learning* (6) which appeared early in 1948. This important book presents the criteria for differentiating between S-R and field theories of behavior; it next proceeds to a detailed exposition of the S-R theories of Thorndike, Guthrie, Hull, Skinner, and the current Functionalists, then to the exposition of the field theories of Koffka, Lewin, Wheeler, Tolman, and other neo-gestalt

[1] This review covers the period from approximately January, 1948 to July, 1949.

theorists. The book is selective rather than encyclopedic, even though specific reference is made to 677 items in the bibliography, and it will surely be a focal point of theoretical discussion and criticism since the author does not have the space or the intent to go beyond a general characterization of the many varieties of theory. Already he has been criticized for his presentation of certain points in Gestalt theory (7) and Thorndikian theory (8), and his weighting of many lines of experimental evidence in favor of Tolman's theory and against Hull's theory will bring the charge of bias.

The basic differences between the S-R and field theories of behavior have been summarized by Hilgard (6) to be: environmentalism versus nativism; the explanation of whole organization in terms of principles which introduce no new process versus the acceptance of whole organization as a given; emphasis on reaction versus emphasis on cognition; use of a mechanical model versus use of dynamic equilibrium as a model; and emphasis on historical causation versus emphasis on contemporary (ahistorical) causation.

Speaking generally, the contemporary S-R theories of learning are the modern expression of the analytic approach of association theory combined with a neo-behaviorism such as recently expounded by Spence (9). Theories involving this approach attempt to predict, and hence explain, the observed correlations between stimulation and response. The theory of learning becomes, therefore, an attempt to explain the observed changes in the tendency to make such-and-such responses when such-and-such stimuli occur. Both the stimulus and response are described in molar, as opposed to molecular, terms in order not to depart too greatly in theory from the level of behavioral description. Thus, the stimuli may be relatively simple, as in the case of a pure tone in the conditioned response experiment, or complex, as in the case of patterned visual stimuli in the discrimination experiment. In fact, no very basic modifications of the approach need occur even though the stimulus is a word or phrase or meaningful object like "mother." In the same way, the response is not a muscle-twitch or glandular secretion as often as it is a motor act, such as lifting ones finger from an electric grid, blinking an eyelid, running down an alleyway, turning to the right, saying the nonsense-syllable VOK, etc.

As for the field theories, they likewise refer to stimulational or environmental variables as the controllable independent variables and they describe the dependent variable behaviors in much the same terms as the S-R theories. The basic difference, then, between S-R and field theories is not in the units of description of the stimulating environment or the behavior of the organism. If such were the case, resolution of the differences between these points of view would be impossible. The basic difference is in the nature of the constructs or intervening variables, chiefly the constructs (10), which are employed in the interpretation of the observed stimulation-organism-behavior relationships. S-R and field theorists, as will be seen later, frequently engage in the same type of experiments

using the same types of problem situations for the study of learning, but with conditions imposed on the experiments in such a way as to, so it is believed, maximize the applicability of their own conceptual structure or minimize the applicability of the opponent's conceptual structure. Therefore, these conceptual structures become critical to the assessment of the recent advances in the psychology of learning, and may also serve to bring into relief the specific theoretical problems of learning on which contemporary experiment and discussion focuses.

The discussion of these basic problems can be materially simplified if attention is directed chiefly to the field theory of Tolman and his associates and to the S-R theory of Hull, Spence, and their associates. This does not, perhaps, do justice to Thorndike and the contemporary functionalists, but these men or their students are either empiricistic and eclectic in their approach to the problems which are emphasized by the Tolman-Hull differences or they definitely lean toward the conceptual structures of the Tolman or Hull groups. Thus, Hilgard clearly favors Tolman's field-theory constructs (6), even though his own research program is neo-functionalist and eclectic. In general, Thorndikian and neo-functionalist students of learning have most in common with the Hull theory as a consequence of their acceptance of Hull's principles of primary and secondary reinforcement as special cases of the Law of Effect and their preference for the analysis of transfer of learning in S-R terms.

The best source of the Hull theory of learning continues to be his *Principles of Behavior* (3) and his subsequent theoretical articles in the *Psychological Review* (11 to 15), although it is being continuously and vigorously revised and extended by others (16 to 19). The theory asserts that learning always involves the establishment or strengthening of connections, or *sHr*'s, between stimulating conditions and responses or acts. This establishment of a habit is dependent on the temporally contiguous occurrence of the stimulus, the response, and a reinforcing state of affairs. The reinforcement may be either primary, in the event that it relieves a basic tissue need of the organism, or secondary, in the event that it is a stimulating cirumstance which, although originally neutral, has been regularly associated with a primary reinforcement. Habit strength increases continuously at a decelerating rate, as the number of reinforcements increases; and the reaction potential of a particular response is a multiplicative function of habit strength and drive strength (Reaction potential equals habit strength multiplied by drive strength). The effective reaction potential of a response is determined by the algebraic summation of the reaction potential and inhibitory influences. These inhibitory influences develop in a manner essentially similar to the development of habit strengths, i.e., through reinforcement, but in this case the reinforcement occurs together with the cessation of response. The moment to moment variation in the effective reaction potential is interpreted by postulating a basic tendency for oscillation. Finally, the obvious need for a principle to cover the spread of the effects of learning in one situa-

tion to many others which are not identical to the training situation is covered by principles of stimulus generalization, which assert that the habit strength or inhibitory strength attached to a particular stimulus will generalize to similar stimuli, the amount of generalization being a function of the degree of similarity. Throughout the development of this theory, there is the implication that the interpretation of more complex forms of learning will not require new principles over and above those adequate for simpler forms of learning, and that all learning requires the operation of those principles.

Tolman's field-expectation or sign-gestalt theory has been restated by him within the year as follows (20). During the course of learning, something like a field map of the environment becomes established as a consequence of the relatively autonomous selective and elaborative operations which the organism performs on the environmental stimulation which comes to it. Specific environmental stimulation is not connected directly to specific responses or acts through either mere contiguity in time or through contiguity plus reinforcement in the Hullian or Thorndikian sense. Instead, the organism associates different environmental events or stimulations one to the other directly and organizes these events into cognitions (insights, hypotheses, cognitive maps). No primary importance is given to reward or reinforcement except as a confirmation or failure of confirmation of a cognition regarding "what-leads-to-what" which has been carried over into action. The establishment of the cognitive relationships is said to occur through togetherness in time, contiguity, plus the organizing property of mind. The process of learning then becomes a series of not necessarily interdependent cognitions, field expectancies, knowledges of what-leads-to-what, with the latitude in specific responses to achieve the goal which is implied in such knowledge. Finally, the theory takes into consideration the phenomena of transfer of learning by assuming that these "cognitive maps" may be limited or comprehensive, "strip-like" or "broad" to use Tolman's terms, depending on the state of the brain, the adequacy of environmental cues, the repetitiousness of training, and the motivating conditions of the organism. Thus, the comprehensiveness of the cognitive map will determine whether the learning is utilized in a generalized form which favors transfer of learning or in a limited "habitual" way which does not favor transfer. Also, the admission that the "map" may be very restricted, as well as broad, permits Tolman to accept the hypothesis that learning in some types of situations may not follow his field-expectancy principles at all, and that there are genuinely different types of learning for which different laws may hold (21).

With these brief summaries of the Hull and Tolman theories as background, it is possible to state the major experimental theoretical problems which have exercised students of learning in 1948 to 1949. These are: (*a*) the question whether there is only one kind of learning or whether there are many kinds which involve basically different variables; (*b*) whether

reinforcement (reward, punishment, effect) plus temporal contiguity are necessary and sufficient conditions for fixation and elimination of responses or whether contiguity plus some organizing property of mind are necessary and sufficient; (*c*) whether the learning process involves a continuous and additive modification of stimulus-response relationships or whether it is properly described as a discontinuous sequence of organizing cognitive acts; (*d*) whether the product of the learning process is a specific stimulus-response relationship or whether it is a cognitive organization or field-expectancy; (*e*) finally, there is the continuing problem of the factors, both motivational and stimulational, which determine the utilization of an acquired mode of response in the same or changed circumstances. All contributions of the year cannot, of course, be summarized under these headings, since many involve the development of the details of either the Tolman or Hull theory, or of some theories regarding important special phenomena. A terminal section will consider some of these developments.

## The Question of Types of Learning

It is evident from the environmentalistic and historical causation biases of the S-R theories that they place a much heavier burden on basic and general principles of learning than do field theories. The insistence that these principles must apply throughout the entire range of behavior phenomena, so skillfully defended in Hull's *Principles of Behavior* (3) and his earlier theoretical papers leads the S-R theorist to reject the introduction of new intervening variables or constructs in order to explain apparent differences between learning processes. Instead, he attempts to derive the "higher" processes such as insight, expectancy, spatial cognition, the fact and effects of stimulus patterning, in terms of principles which are necessary and sufficient for presumably simpler forms of learning. On the other hand, the field theorist, because of his willingness to postulate novelties achieved through the organizing properties of mind, such as insight, expectancy, cognitive maps, finds in a pluralism of learning process no theoretical obstacle or frustration.

The bias of the S-R theorist favoring a monistic theory of learning is well represented by Hull's insistence (3) that the reinforcement theory applies equally as well to classical conditioned response (CR) learning situations (Pavlov) as to trial and error learning situations, even though the classical CR learning has been accepted by Tolman (22) and others as a unique case of signal learning through mere contiguity. Kendler & Underwood (23) have recently re-examined the earlier arguments of Maier & Schneirla (24) for this basic distinction between classical CR learning through mere contiguity and trial and error learning in which the reward or punishment is instrumental in selecting the correct response. Kendler & Underwood maintain that the evidence cited by Maier & Schneirla is not crucial and that the reinforcement form of S-R theory is applicable to both types of learning situation. In short, they support Hull

in maintaining that only one set of necessary and sufficient conditions is required for these apparently different forms of learning.

On the other hand, Tolman, as a proponent of field theory, has suggested that there may well be a number of different kinds of learning, some of which fit the laws of the S-R theorist and some of which require the postulation of field-expectancies and field-cognitions (21). A strong case is again made for at least a tentative pluralism of learning processes and for the acceptance of this pluralism as a solution to the theoretical conflicts of his and Hull's theories of learning, as well as the differences between these and the theory of Guthrie. However, it is doubtful that Tolman's classification scheme, or any other, will resolve the differences inherent in S-R theory and field theory merely by the expedient of embracing both. In fact, only a year earlier (20), Tolman himself proposed that what appeared to be response learning and place learning were perhaps only different points on a continuum of field-cognitions or cognitive maps which varied with respect to the width of the map.

Hilgard (6) accepts the plurality of learning processes as a necessary methodological postulate and argues for a neo-functionalist approach in which the primary emphases are (*a*) the need for miniature experimental-theoretical systems in which there is an appropriate restriction of generalizations to the limited operations involved in the learning process being studied, and (*b*) the need for pliability and eclecticism in the basic concepts which are employed (e.g., stimulus, response, expectancy, psychophysiological variables, etc). In this view, the ultimate fertility and adequacy of stimulus-response concepts and field concepts, or some combination of them, is made a matter of the empirical extension of knowledge through systematic experimentation.

Other publications pertinent to this problem, are Allport's (25) criticism of the tendency of theorists to build on mechanical models and on studies of animal behavior. This criticism implies that the differences in kind of organism and in developmental level introduce hiatuses in mental process which cannot be bridged by general theoretical principles. Brownell (26) expresses the same type of dissatisfaction with present-day learning theories in so far as they lack applicability to educational practice. Seward (27) has given a detailed reply to Allport's thesis particularly as it concerns the assumption that animals, in contradistinction to human beings, do not have symbolic ability. A careful and thoughtful analysis of 10 types of problems which purport to measure symbolic function in animals led to the conclusion that six types reveal symbolic function in animals when certain requirements are met, and to the statement of the postulate that "the conative and cognitive processes of humans and other species belong on a continuum, varying only in complexity."

The fruitfulness of an experimental approach to the question of types of learning process has been demonstrated in the epochal studies of Harlow (28) on discrimination learning in monkeys. Harlow has shown that the monkey progresses in an orderly fashion through cumulative experi-

ence from a trial-and-error animal to an insightful animal. For example, in one experiment which involved the solution of 344 discrimination problems, where the discrimination was always based on object-quality, the monkeys made the correct reaction on the second learning trial only 53 per cent of the time during the first eight problems, but made the correct reaction on the second learning trial 97 per cent of the time during the last 56 problems.

Harlow rightly stresses the failure of stimulus-response theorists to manipulate the experience variable as it becomes involved in learning sets or learning how to learn, even though they constantly emphasize an historical approach to learning. Likewise, the field theorists have been at fault in being too ready and willing to postulate insight and hypotheses as innate and unique properties of the organism without controlling the experience variable. Learning how to learn has, of course, long been recognized as a factor in human learning, and students of animal learning are usually careful to give the animal preliminary indoctrination of some type, but the systematic significance of this factor in the controversy over types of learning processes has never been so clearly demonstrated.

The assessment of contemporary history is always dangerous, but it will surprise many students of the learning process if Harlow's studies do not represent a critical integrative departure in learning theory and the beginning of a long series of experimental studies designed to determine the essential character and conditions of learning sets. The most conservative evaluation credits these studies with showing a fundamental fallacy in experiments and theories which are ahistorical in methodology or postulate or, as in the case of stimulus-response studies, have too narrow control and interpretation of the historical antecedents of the individual's behavior. In passing, it may also be noted that Harlow's studies add materially to Seward's (27) defense of the fruitfulness of animal research against the criticisms of Allport (25).

The year ends, therefore, with a new and stimulating experimental approach to the fundamental question of types of learning process which promises to relate the types to a dimension of the experience of the organism; the reiteration of the eclectic functionalist's insistence that distinctions between the operations employed in different learning situations must not be casually ignored by theory; a new defense of reinforcement learning as the necessary condition for classical CR learning as well as trial and error learning; and new protestations that there are fundamental and irreducible differences between animal and adult human learning, and between different learning processes in the same species.

## Reinforcement Versus Field-Cognition

At the present time, the theory that asserts that reinforcement is a necessary condition for learning is faced with two responsibilities, one defensive and the other constructive. It must defend itself, either by new experiments or by developments in theory, against the implication of the

classical "latent learning" experiment that the organism can learn, through processes of perceptual or cognitive organization which depend only on temporal contiguity, in the absence of a temporally contiguous reinforcing state of affairs. In the second place, it must continue vigorously to pursue the explication of the mechanism or mechanisms of reinforcement.

The question whether reinforcement is a necessary condition for learning must be distinguished from the question whether reinforcement is influential in determining the rate of acquisition of specific behaviors. Postman's (29) recent definitive review of the literature on the law of effect leaves little doubt that reinforcement conditions have an important influence on the course of behavior modification. Furthermore, field theorists such as Tolman accept rewards and punishments as regulators of performance and as emphasizers in perception (22).

Since the reinforcement theory asserts that all learning, whether it be between stimulus and response or between spatial components in a perceptual organization, must involve a reinforcing state of affairs, it cannot stand without major modification if "latent learning" is demonstrated under conditions which preclude reinforcement. In the ideal "latent learning" experiment, as devised to demonstrate sign-gestalt or field-cognition learning, the organism is placed in a situation where certain cognitions of what-leads-to-what (e.g., right turn in a T-maze leads to box with food) may occur, i.e., all components can be perceptually discriminated, but where the occurrence of such conditions will not be temporally contiguous to either primary or secondary reinforcement. If on later tests it is determined that such cognitions of what-leads-to-what must have occurred, then reinforcement cannot be a necessary condition of learning. Obviously this experiment has much in common with the "incidental learning" studies with human subjects, and has been stressed in recent theoretical writings largely because of the use of white rats in which it may be presumed that the presence of subtle autonomous and symbolic motives is minimized, thus increasing the probability of a crucial study. It is worthy of note that the latent learning experiment may, under ideal conditions, disprove reinforcement theory, but the failure to demonstrate latent learning does not prove reinforcement theory (30).

The numerous latent learning experiments and their interpretations prior to 1948 have been summarized and interpreted in Hilgard (6) and Spence & Lippitt (31). Certain latent learning experiments in which positive results were achieved have been reviewed in Tolman (20) and made the major basis for a reiteration of his theoretical approach. Hilgard concludes that the occurrence of latent learning is securely established as an experimental fact; that it is one of the strongest supports of the Tolman theory; and that it is one of the major evidences against reinforcement theory. However, since this publication there have been five reported studies of latent learning, of which four have been negative.

The first study to appear was by Kendler (32). He used a single-unit

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T-maze in which the left alley was painted black and led to water while the right alley was unpainted and led to food. During the training trials, six animals were highly motivated for food, four animals were highly motivated for water, and all animals had equal experience with the two goal boxes. During half the test trials, the animals were shifted to the drive for which they had been satiated during training, and during the other half they worked under the same motivation as during the training trials. All animals continued to choose the alley which led to the appropriate reward object during training, regardless of the current motivation. This experiment was a repetition, except for the differential coloring of the alleyways, of the Spence & Lippitt (31) study which had the same result and which led Hilgard (6) to say, "It is necessary for the believer in latent learning to assert that the thirsty animals were not interested in paying attention to food, and hence did not form the appropriate cognitive structure . . . ."

Leeper (33) then asserted that the Spence & Lippitt (31) and Kendler (32) studies required a revision of the sign-gestalt interpretation to the effect that the goal-objects presented to the rat when not motivated specifically for them should be perceived discriminatingly if latent learning were to occur. This criticism and evaluation was answered by Spence & Kendler (30) on rational grounds, and by Kendler & Mencher (34) on experimental grounds. The latter performed a striking experiment in which, during the training trials under thirst motivation, the rats were required to discriminate the one cup of five in the goal box in which the desired goal-object (water) was present. In the other four cups in one goal box there was food, and in the other four cups in the other goal box there was nothing. The cups were positioned in such a way that the rat was forced to react to the contents of the cups in order to satisfy its thirst motivation, thus insuring the discrimination (and rejection) of the food cups and the empty cups during the training trials. The test trials gave negative results with respect to the formation of cognitions of food location. Grice (35) performed a similar experiment with the T-maze and not only tested the latent learning by a recall criterion, but also used a relearning criterion. Again, the results were negative. Walker (36) used a somewhat different technique involving a distance discrimination based on thirst motivation, with the animals exposed to quantities of food if they chose the longer of the two paths to water. When the motivation was changed to hunger all animals continued to make the turn to water, and the group that had been exposed to food in the longer alley learned to go to that alley for food no more readily than the group that had not been exposed to food in the longer alley.

These studies involved in every instance the employment of strong motivation during the training trials in which the cognitions were to be acquired. On the other hand, Meehl & MacCorquodale (37) and Seward (38) have reported evidence for latent learning under conditions which involve satiated basic motivations. It would appear that conditions of

satiated drive favor the occurrence of latent learning, but that it cannot be demonstrated under strong drive conditions. It would also seem that this empirical generalization has been anticipated by Tolman (20) in his theory that "cognitive maps" may be severely restricted by strong motivation, but be broad under conditions of moderate or low motivation. This represents a modification of the sign-gestalt theory which has been forced, for the most part, by defenders of the reinforcement theory, and creates a theoretical problem for them just as the repeated verification of latent learning under satiated drives continues to harass the reinforcement theory.

However, evidence for learning under satiated drive conditions is not crucial for reinforcement theory if it can be demonstrated that the situation involved secondary reinforcing stimuli. Field theorists frequently neglect to consider the power gained by reinforcement theory through the investigations of secondary, or derived, reinforcement and secondary, or derived, motivation. Although the reinforcement theory starts with the postulates of primary reinforcement (need-reduction) and primary drives (needs), it is obviously required to introduce secondary motives and reinforcements, since so much learning, especially human, is far removed from primary drives and need-reductions. To bridge this gap there has been introduced the notion of the secondary reinforcing stimulus, and it has been postulated (3) that the regular occurrence of a neutral stimulus (*e.g.*, tone) in conjunction with primary reinforcement (*e. g.*, food) will result in the neutral stimulus acquiring the reinforcing properties of the primary reinforcement. This theory has received further development in recent months by Miller (39) and May (40) in order to encompass the acquisition of acquired drives as well as derived reinforcements. All such developments, and their experimental supports, provide a rational basis for the explanation of instances of "latent learning" in terms of reinforcement, even though the primary drives are satiated and primary rewards are absent. At the same time, these developments have permitted a clearer understanding of the role of primary reinforcement in learning which involves periods of delay between the occurrence of the correct response and the occurrence of the reinforcement.

The basic phenomenon of secondary reinforcement has been demonstrated in several recent studies. Estes (41) repeatedly presented a tone and food to hungry rats, and later established the bar-pressing response in a Skinner box using food reward. In later test trials, it was found that the rats would operate the lever in the box with greater frequency when both tone and food occurred as reinforcement than when food alone occurred. Thus, the secondary reinforcement provided by the tone was shown to be in addition to the primary reinforcement provided by food. It was also shown (42) that this secondary reinforcing stimulus was effective even though the primary drive was changed from hunger to thirst, but that it only operates when some organic drive is strong enough to instigate activity. Denny (43) used a T-maze which prevented extra-maze

cues and found that learning was considerably slower when the two end boxes were identical than when they were visually different. This result was predicted on the assumption that the wrong end box, without food, gives some secondary reinforcement to the wrong response when it is identical with the correct end box in which the food is eaten, and thus makes response differentiation more difficult. Ehrenfreund (44) also demonstrated the extreme sensitivity of these secondary reinforcing factors in visual discrimination learning. In his study, the wrong goal box contained, under one condition, an empty food cup identical with the one in the correct goal box. Under a second condition, this food cup was not present in the wrong goal box. Learning was retarded when the food cup was present in the wrong goal box, thus confirming the hypothesis that it served as a secondary reinforcement for the wrong response.

The intimate involvement of such secondary reinforcing stimuli in all learning is also a conclusion forced by recent experimental and theoretical work on the gradient of primary reinforcement. Hull has suggested (3) that primary reinforcement could not operate to strengthen an S-R relationship separated from it by more than 30 sec., and that the demonstrated occurrence of learning after longer delay intervals must be attributed to secondary reinforcement through the similarity of stimuli at the moment of response and at the moment of primary reinforcement. Spence (45) has developed this idea further, and suggests that all strengthening of an S-R relationship through primary reinforcement may require simultaneity, with the apparent efficacy of reinforcement after any interval of delay being attributed to secondary reinforcing stimuli, especially kinesthetic stimuli. Grice (46) has followed this suggestion in three experiments with the visual discrimination problem in rats. Since spatial cues are eliminated, he predicted that the maximal delay in primary reinforcement which permitted learning would be very short. This was found to be the case, and it was further shown that learning with delayed reward became easier when the goal boxes for the correct and incorrect visual stimuli were distinctively colored and also when the rat was forced to make some distinctive motor adjustment in reacting to the correct visual stimulus. These studies, when combined with earlier studies (3, 45), clearly support the role of secondary reinforcement in learning which appears to occur without relevant reward or with relevant reward removed in time. It is pertinent, therefore, to the whole problem of "spread of effect" in human learning, and it may be that the gradient of symbolic reward and punishment which is so basic to Thorndike's theory of learning (4, 5, 6, 29) may have some validity in spite of the accumulation of negative evidence (6, 29) and the recent negative results of Jenkins & Cunningham (47) and Smith (48).

Before too much confidence can be placed by reinforcement theorists in secondary reinforcement and secondary motivation as explanations of apparent learning without reinforcement, much experimentation is required to determine the conditions which affect the acquisition of such

reinforcing and motivating properties by originally neutral stimuli. Miller (39) has shown that stimuli present during electric shock may exhibit, when presented alone, the character of a drive in that they motivate the learning of a new habit; and he has also demonstrated that there is a relationship between the strengths of the acquired drive and the primary drive on which it is based. May (40) has performed similar experiments and has demonstrated that the acquired drive may affect performance in other situations through transfer in accordance with the principle of stimulus generalization. Other important complications of the derived reinforcement and derived motivation problem are presented in the studies of Young (49) and Farber (50).

With reference to the conditions of establishment and utilization of secondary reinforcement, Saltzman (51) has made a direct attack on the problem by determining the conditions under which preliminary training of rats with food reward in a black or white goal box with distinctive movement requirements would result in learning a T-maze under hunger drive when the only reward was the black or white goal box.The efficacy of secondary reinforcement was again demonstrated, and some evidence was gained regarding the relative effectiveness of different procedures for establishing and maintaining the efficacy of this secondary reinforcing stimulus.

In summary, the phenomenon of "latent learning" still remains as an apparent exception to the reinforcement theory of learning, although recent experiments have suggested that latent learning occurs only under conditions of satiated primary motivation. On the other hand, reinforcement theory is being continually strengthened by research on secondary reinforcement and secondary motivation which demonstrates their widespread involvement in the learning process. Furthermore, some progress is being made in the description of the conditions under which such derived motivations and reinforcements are developed and utilized, and further progress may be confidently anticipated. It seems unlikely at this time that the reinforcement principle will be abandoned on the basis of any one or several experiments; instead, it appears that attacks on the principle only lead to more fruitful explorations of its complications.

### Continuity Versus Noncontinuity in Learning

As has been previously indicated, the S-R reinforcement theory and the field theories differ on another crucial postulate regarding the learning process, namely, whether the learning process involves a continuous modification of S-R relationships or whether it is properly described as a discontinuous sequence of organizing acts. This divergence of viewpoint has come to be known as the continuity-noncontinuity controversy. Although the controversy had its origin in experimental analyses of discrimination learning (52, 53), it must be considered fundamental to all learning theory, since the continuity postulate is explicit in the general

theories of Hull (3) and Thorndike (4), and the noncontinuity postulate is explicit in the general theories of Tolman (20) and Lashley (54, 55).

Early criticisms of the continuity postulate emphasized the systematic and nonrandom character of presolution behavior in discrimination learning (56), but this was shown by Spence (57) to be compatible with trial-and-error theory. The focal points of the controversy in recent years have been: (*a*) tests of positive or negative transfer in discrimination learning after reversal of the reward relations of a stimulus before the organism shows evidence of systematic behavior with reference to the original stimuli; (*b*) tests of positive or negative transfer when a new problem involves reaction to a stimulus component which has been irrelevant in prior training; and (*c*) analyses of the correlation between the frequencies of prior rewards and punishments of particular S-R relationships and the frequency of errors in subsequent training in which those relationships are correct. If reversal of cues during the presolution period leads to negative transfer in the subsequent learning, the continuity postulate is supported. Similarly, the continuity postulate is supported by evidence for either positive or negative transfer in new learning which involves positive or negative response, respectively, to a previously irrelevant component of a stimulus complex, and by evidence for correlation between the reinforcement history of an S-R relationship and its subsequent ease of learning.

The many conflicting experimental results on these three lines of evidence have been reviewed and analyzed in detail by Blum & Blum (58). In the case of reversed pretraining, they conclude that studies which apparently support the noncontinuity theory involve massed trials, punishment, and difficult discriminations, and that the opposite experimental conditions give results favoring the continuity theory. They present a cogent argument for the interpretation of the noncontinuity results in terms of the Hullian principles of stimulus generalization and inhibition, thus making them compatible with continuity theory.

This analysis is given further support by the recent study of Ehrenfreund (59) which demonstrated that the effect of cue reversal in pattern discrimination in rats depends on the vertical position of the stimuli in the Lashley jumping box. When the stimuli were high in the window—a difficult task because of the unusual visual fixation required of the rat—reversal during the presolution period had no effect on later learning; when the stimuli were low in the window—a less difficult task—reversal resulted in retardation of later learning. Another recent study, by Grice (60), which likewise supports the continuity theory, will be considered later in the discussion of stimulus generalization. Finally, Prentice (61) has contributed an important new direction in this approach to the continuity problem through the use of human subjects from whom verbal reports were available to aid in determining the time at which systematic correct solutions began to appear. When the appropriate stimulus cues

were reversed prior to such systematic solutions, it was found that the reversal group required many more trials to complete the learning than the nonreversal group. This result is distinctly contrary to the expectations of the noncontinuity theory, although it presents certain problems of interpretation for the continuity theory.

A further development of the continuity thesis is that aspects of a stimulus (e.g., shape) may become attached to particular responses through reinforcement even though they are not relevant to the solution of the current discrimination problem. Lashley (55) and Spence (62) have previously reported conflicting results. Blum & Blum report (58) a repetition of Lashley's experiment with some modifications. They obtained results opposite those of Lashley and favorable to the continuity interpretation. After positive training on a large circle and negative training on a small circle, rats were tested and then trained on a large erect triangle and a small inverted triangle. In the initial tests the rats preferred the large erect triangle, and this response tendency was reinforced 200 consecutive times. The final test was made on medium-sized triangles, one erect and one inverted. The rats reacted positively to the erect triangle, thus demonstrating that they had learned to discriminate the forms, despite a set presumably only for size discrimination. Blum & Blum also present a correlation analysis of frequencies of reinforcement and error scores which substantiates the continuity theory.

Although recent evidence seems to favor the continuity postulate, either directly or through interpretability in terms of other principles encompassed by the reinforcement theory, the impact on this problem of Harlow's (28) data on the formation of learning sets in discrimination learning has not yet been assessed. His studies give ample evidence of orderly and lawful discontinuity in the discrimination learning processes of monkeys and require a reformulation of the experimental approach to the problem even though they are not necessarily incompatible with S-R reinforcement theory.

### Place Learning Versus Response Learning

A fourth point of issue between S-R reinforcement theory and sign-gestalt theory which looms large in the current literature concerns the question whether the product of the learning process is a specific stimulus-response relationship (response learning) or a cognitive organization or field expectancy (a place or directional disposition). The contemporary interest in "place learning" experiments was stimulated by the studies of Tolman, Ritchie & Kalish (63, 64) which purported to show that the rat is not moving through a maze according to a fixed sequence of movements, such as reinforcement theory is said to presume, but is capable of varying its behavior with variation in the environmental conditions, as though it knows where the goal is. As Hilgard (6) has pointed out, such place learning experiments are but one of several lines of evidence for "sign learning" as contrasted with "response learning." Tolman (20)

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places heavy emphasis on these experiments, which he now calls "spatial orientation experiments," in support of his own theoretical position. Up to now there has been no detailed theoretical reply to these claims by reinforcement theorists, although there have been several critical experimental studies.

The major contention of Tolman and his co-workers appears to be that spatial orientation dispositions (place or direction) are more primitive than response dispositions and are, therefore, more readily established. In one type of study, which was introduced by Tolman, Ritchie & Kalish (63), rats are trained in a relatively simple maze and are then given an opportunity, after the training pathway is blocked, to choose one of a number of paths irradiating from the starting box. One of these new pathways leads to the original goal box and the visual cue (e.g., bright light) which has been associated with it during training. A place disposition is presumed to be revealed when the rats choose the pathway leading to the goal box and cue; a response disposition is presumed to be revealed when the rats choose pathways near the blocked training pathway. The original findings which favored the prepotency of place dispositions have not been confirmed in studies by Gentry, Brown & Kaplan (65), and Gentry, Brown & Lee (66). In an interesting modification of this design, Wilcoxon & Waters (67) presented two "short cuts" from the starting box in a modified single-T-maze, one leading to the goal box and the other leading to a symmetrically placed incorrect box. No preference was exhibited for the correct short cut. In another study by Ritchie (68), there was again failure to find that the rats chose the path pointing directly toward the former food place, but there was evidence for a strong "direction disposition" which resulted in the rats choosing the first path pointing in the general direction of the food box. This finding is not necessarily inconsistent with S-R reinforcement theory, since it may represent an anticipatory turning response.

A second method designed to determine the relative strengths of response and place dispositions involves their direct opposition during training and test. The animals are first trained to turn to the right in a simple T-maze to reach the food box. The leg of the T is then shifted so that it is necessary for the rat to turn to the left to go to the food box and toward the orienting visual cue associated with it. The first study of this type, by Tolman, Ritchie & Kalish (64), gave evidence that the place disposition was stronger than the response disposition. However, recent studies by Blodgett & McCutchan (69, 70) have given contrary evidence. Walthall's study (71) indicates that the determinants of these dispositions are indeed complex. It would seem that once again an experiment which is put forward as "crucial" for theory (e.g., the latent learning experiment, the training reversal test of continuity) yields contradictory results under different conditions and becomes an area of research in its own right. At the present time there is no clear evidence in favor of Tolman's original contention that spatial orientation dispositions are more

primitive and more readily acquired than response dispositions, even though this hypothesis remains basic to his discussion of learning in terms of the formation of "cognitive maps in rats and men" (20). On the other hand, this renewal of interest in spatial orientation may have the salutary effect of stimulating S-R reinforcement theorists to examine the applicability of their theory to this hitherto neglected problem.

## Transfer of Learning

The problem of transfer of learning is the problem of the utilization or failure of utilization of previous learning under conditions which differ in some respect from the conditions extant during the original learning. Traditionally, the problem has been approached in two ways. In the first, which is the more analytical, determinations are made of the strength (frequency, amplitude, latency) of the previously trained response when some aspect of the stimulus has changed, or adaptive variations of a trained response are observed under constant stimulating conditions. Such studies have led to the formation of laws of associative assimilation, or the descriptive concepts, stimulus generalization and response generalization (72). In the second approach, which is usually less analytical with respect to the stimulus and response components involved, the principles of transfer are sought through determining the effects of prior learning on the rate of acquisition of some new act which bears some definable similarity relationship to the original learning. This approach has led to analyses of the conditions of positive and negative transfer effects. Although transfer, like retention, may be measured legitimately by either such recall or relearning measures, the principles resulting from measures of recall are generally considered more basic. As a consequence, primary stimulus generalization has been considered by Hull (3) as basic in his theory of learning, and he further asserts that the strength of response to an altered stimulus is an increasing function of the similarity of the training and test stimuli. This principle is widely used by S-R theorists in the interpretation of behavior.

In 1946 Lashley & Wade (73) made a frontal attack on this principle of primary stimulus generalization, denying that the gradients which had been reported were related to a basic property of the nervous system or to habit strengths established during learning, and insisting that they were due to failures of attention, variable stimulation, or prior differential training. They cited their own failures to obtain either positive or negative transfer effects in a discrimination learning situation when prior learning was given on one of the to-be-discriminated stimuli. This view is consistent with the view that the learning process is noncontinuous, which has been discussed earlier.

These criticisms were quickly countered by Hull (12). However, the failure of Lashley & Wade to find stimulus generalization after training on single stimuli called for new investigations employing their methodology, especially since they were in the logically indefensible position of accept-

ing the null hypothesis on the basis of a small number of observations. Two well controlled studies modeled after these of Lashley & Wade have been reported during the last year. Both yielded results which confirm the expectations of the theory of primary stimulus generalization and contradict the Lashley-Wade results. Grice (60) demonstrated that prior learning on a single stimulus produced a significant effect on later size discrimination in the case of rats. Grandine & Harlow (74) demonstrated stimulus generalization gradients in learning height and brightness discriminations by monkeys following training on single stimuli. Although one grants that these experimental contradictions of the Lashley-Wade findings do not prove the Hull theory (74, 75), they do call in question the negative evidence which was given such weight in the critical arguments of Lashley & Wade.

To date, this controversy has had the salutary effect of bringing to the fore the possibility that stimulus generalization gradients may be secondary, rather than basic, phenomena. Thus, Razran (75) has been stimulated to an intensive critical review of the evidence on stimulus generalization in conditioned response learning. He concludes that the evidence does not favor Pavlov's irradiation theory or Hull's specific theoretical quantification of the gradient of primary stimulus generalization. Neither do they favor unequivocally any or all of Lashley & Wade's suggested interpretations. He concludes by presenting his own theory which (*a*) affirms the existence of true conditioned response generalization, while recognizing the existence of pseudogeneralization which conforms to the expectancies of Lashley & Wade, (*b*) rejects the Hull theory that generalization is the effect of latent habit strengths laid down during original learning, on the ground that generalization develops only during the subsequent testing for it, and (*c*) asserts that all gradients of stimulus generalization are not continuous functions of stimulus similarity, but are the result of a crude similarity-dissimilarity scale used by the organism for rating or categorizing purposes. No evidence is marshalled in support of these points, but they, together with the Lashley-Wade interpretations, will become the targets of S-R theorists. It is to be hoped that this heuristic value of the controversy will result in some increase in the number of systematic studies of the relationships between the presence and form of the generalization gradient and the dimensional characteristics of the organism, the stimulus, and the response. Perhaps this would lead to a revival of work on the relatively neglected phenomenon of response generalization, on which one study appeared during the year (76).

To date, those who have studied the conditions of transfer of learning by studying transfer effects have approached the problem systematically, and are less prone to controversy about postulates. Osgood (77) has explicated the traditional functionalist S-R interpretation of transfer, with special reference to retroactive inhibition, and has formulated a series of laws of transfer effect in terms of stimulus and response similarity. Gagne, Foster & Crowley (78) have made a careful and fruitful analysis of

the many methods which have been used to give quantitative expression to measures of transfer of training. Also, Gagne & Foster (79) have presented important experimental support for an analytic S-R approach to the prediction of the transfer effects in complex skills which may be expected from practice on component skills.

## Retention and Forgetting

For reasons which are not clear, the major theories of learning have not incorporated the major advances in the theory of retention and forgetting which have occurred in the past 25 years. For example, Hull (3) and Thorndike (4) accept disuse as a major factor in forgetting, even though this principle has been under severe attack for many years (72). Likewise, field theorists have not formulated their interpretations of learning with any explicit recognition of current theories of forgetting.

Meanwhile, experiment and theory on retention and forgetting continue to be prominent in the literature. Certain recent findings extend and correct the earlier theoretical developments. Of first importance among these are the electroconvulsive shock (ECS) studies which seem at the moment to indicate a need for revival of a perseveration or consolidation hypothesis in the interpretation of retention and forgetting. Duncan (80) has reported a study of rats trained to avoid a charged grid on a one-trial-per-day schedule with ECS administered 20 sec., 40 sec., 1 min., 4 min., 15 min., 1 hr., 4 hr., and 14 hr. after each trial. Retardation of learning by the shock decreased continuously with lengthening of the interval before shock, until at 1 hr. or longer there was no significant effect. In control studies with shock to the hind legs, only those rats shocked 20 sec. after each trial showed retardation of learning. These results suggest that a consolidation process follows practice for at least 15 min. but not as long as 1 hr. This conclusion, if substantiated, will have major importance for theories of reminiscence and retroactive inhibition.

Other recent studies with ECS do not bear directly on this problem, but they have some relevance. For example, Townsend, Russell & Patton (81) found that shock immediately after learning had a detrimental effect on relearning after 24 hr., but did not affect a second relearning after 30 days. This would be expected if ECS disrupted only that learning which occurred within 1 hr. before it, and had no permanent deteriorating effect on the nervous system. Whether this latter is true is a matter of some difference of opinion at this time. Hayes (82) has rejected the hypothesis of brain damage as an explanation of his results with rats, while Porter, Stone & Erickson (83) have concluded that there may be damage when ECS is applied to infant albino rats. Whether or not ECS permanently reduces the capacity to learn may be a function of the difficulty of the learning task (83, 84). In a free-choice Dashiell-type water maze, Muhlhan & Stone (85) have found temporary, but not permanent, maladaptive behaviors resulting from ECS.

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In recognition of the importance of the phenomena of reminiscence and distribution of practice for general theory of learning and retention, Buxton and his co-workers (86, 87, 88) have repeated earlier studies of reminiscense in rote serial learning with a number of procedural variations. Alper (89) has also contributed to this analysis. Studies of the phenomena resulting from distributed practice in motor learning and their theoretical interpretation have been made by a number of investigators (90 to 93). In this connection, Solomon's earlier review (94) and analysis of the influence of work on behavior, especially his analysis of the applicability of Hull's theoretical concepts to the interpretation of distribution of practice and behavior variability, has major significance for learning theory, and supports the fruitfulness of the S-R theoretical approach.

Twenty-five years ago retroactive inhibition was an oddity of the laboratory. Today, the phenomena to which the concept (and the related concept of proactive inhibition) refer are considered basic to the understanding of forgetting and are frequent objects of experimental study. Even though as noted above, a concept of consolidation of memory traces may need acceptance, the inhibitory processes in retention resulting from the learning of prior (proactive) or subsequent (retroactive) behaviors remain dominant factors in forgetting. Recent studies have attempted to test some of the consequences of the theory that retroactive inhibition, as distinguished from proactive inhibition, involves an unlearning (experimental extinction?) of the original learned activity as well as competition of response tendencies at the time of testing for retention (95 to 98). Although a transfer theory of such inhibitory influences, which implies acceptance of the empirical principle of stimulus generalization, continues to be preferred, the exact form of the theory is still a matter of debate (95, 98). In addition, the proper dimensions of description of similarity of both stimulus and response remain critical. Osgood (99) has made a fruitful statement of the similarity relationships involved in transfer of learning and retroactive inhibition, and has presented (100) new data on the meaningful similarity factor in such inhibitions. A valuable correction to the attribution of too much forgetting variance to the inhibitory transfer factors has been provided by Irion (101), who has shown that much "forgetting" as measured by the usual methods is due to lack of warming up to the task.

## Summary

Many isolated studies which may loom large in future evaluations of the year 1948 to 1949 have been neglected in this review. The psychology of learning is such a vast field, both in theory and in actuality, that no temporal review can even approximate comprehensive coverage. The student of the psychology of learning should be sympathetic, the student not so inclined should remember that what has been reviewed here is but a portion of the problem.

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# VISION

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The support of research by Federal agenices continues to show its effect upon the field. A general review (1) of work sponsored by the Committee on Medical Research during the recent war has become available and should be added to the summary descriptions of war work cited in the two preceding annual reviews of vision (2, 3). Many articles appear bearing acknowledgment for support from Federal agencies. There is no question that most of these articles are worthwhile contributions, and that our knowledge of vision has profited. But at the same time there is a neglect of some important problems. Certainly, as the number of titles in this review may indicate, the theory for some topics, such as areal and temporal summation, inhibition, contrast, and the constancies, is receiving attention from relatively few psychologists. This neglect may be the consequence of the pressure for research more likely to have applications.

Those who are anxious for assurance that vision research will thrive under subsidy can find comfort in examining a book (4) on color which has issued recently from the Eastman laboratories. Evans' book becomes an excellent complement to Wright's recent text [see (2)], and the two books together constitute the best presentation of color since Helmholtz.

## Methods and Equipment

Considerable space in almost every article is devoted to the methods for measurement. On the one hand, this fact is testimony to the good training and experience of the authors; anyone who has attempted any vision measurements realizes well the degree to which his data depend upon the details of his techniques. But on the other hand, the same fact is a demonstration of our need for further research on general methodology. Only one contribution in this realm has appeared in the past year. Blackwell reports (5) on the success with which various measurement methods for brightness discrimination provide threshold values which are independent of variations in procedure, in instructions, or in observer attitude.

Several interesting developments of equipment have been reported. The following paragraphs are noted on general purpose devices for flashes, screens, and recording. Some more specialized apparatus will be discussed in context later.

*Short flash generators.*—Flash tubes with peak light output of several million lumens, and with a very rapid decay, have been on the market for some time, and their applications in studying the mechanisms of acuity, e.g., are reported (6). Glow modulator tubes, whose duration of firing can be regulated because their light output varies in an essentially linear

manner with current, show considerable promise for applications demanding precise control of the duration of a small intense source. Associated equipment for making them into flexible light flash generators has been described (7). Unfortunately, neither of these types of lamp yet lend themselves properly to settling the old question of the time-intensity relationships for very brief stimulation. The flash tube has an after decay and its firing cannot be regulated to yield a burst with duration controlled precisely, and the stability of the glow-modulator tube is not determined. Attempts to construct electro- and magneto-optical shutters utilizing the polarization properties of certain crystals continue to be made, but progress is slow (8). We venture to guess that the most satisfactory solution for controlling duration will be in the improvement of mechanical shutters for occluding and exposing a steady source.

*Phosphor screens.*—Leverenz (9) summarizes some of his recent material on light-emitting phosphor screens. Developments of luminiscent screens should be of interest to anyone concerned with the construction of a stimulus surface, inasmuch as such screens closely approximate the ideal requirement for presenting an infinite number of point sources.

*Aids to recording and measurement.*—The photomultiplier tube is reported effective for measuring brightnesses well down in the scotopic range (10) and one study of its application for recording light transients shows satisfying stability (11). A device permitting observation of the pupil in total darkness has been described (12). Finally, it is appropriate that in this section we mention an apparatus of the type that everyone measuring thresholds will appreciate. It permits the determination of visual thresholds without an experimenter (13). Presumably the subject is indispensable.

## Research on Visual Functions

*Depth perception.*—Interesting observations have been reported on the landing errors made by aircraft pilots wearing masks which eliminated binocular vision without appreciably reducing the total extent of the visual field. The use of binocular clues in perceiving altitudes at 500 to 800 feet is demonstrable (14). Coupled with this note on the importance of binocular clues should be the finding (15) that depth can be sensed with field exposures as short as one-sixtieth second, thus showing that proprioceptive changes during fixation are not required for stereoscopic depth perception.

Instruments for measuring depth performance (16, 17) have been described, and there have been two treatments of the geometry of binocular vision underlying depth discrimination (18, 19). Graham *et al.* (18) reason that, under certain conditions of stereoscopic observation with reticles providing multiple reference lines, there should be several false values of range at which the visual target will appear aligned in the same plane as the reticles. Their arguments have been borne out in experimental data.

XIIM

Berry has contributed some quantitative relationships among vernier real depth, and stereoscopic depth acuities which indicate that real and stereoscopic depth thresholds are essentially identical. Moreover, the threshold of depth discrimination (about 2 sec. of visual angle) is too low to be accounted for as a summation of vernier discrimination by the two eyes (20). The influence of illumination level upon stereoscopic acuity has been investigated (21). Acuity is shown to increase with illumination. There is a discontinuity of rod and cone functions and an indication that depth can be sensed at light levels below cone thresholds. Lateral separation of the visual target from the comparison standard is an important factor, though in the two situations reported the effects of separation are different. In one report (22) of the accuracy of stereoscopic settings we are told that, as the separation between the target and the fiducial line is increased, precision decreases; and in the other (23), with observations of real depth, an optimum separation of about 19 min. of visual angle was manifest. It may be significant that in the latter study exposures were brief so as to exclude ocular movements during observations. (24).

For equipment of the Howard-Dolman type it has been shown that when one rod is slightly larger than the other, there is a slight displacement in the position of the two rods for subjective equality of distance. The displacement is in the direction of size equality (25). Results with the brightness and color of one rod varied are reported (26) but it is a fair guess that the data will prove amenable to interpretation on the basis of a shift to equality for the associated apparent size change. Additional material on aniseikonic effects upon distance discrimination has appeared (27, 28). Eyedness of the observer seems to influence the position of subjective equality (29).

Monocular movement parallax thresholds have been investigated at the Columbia laboratories. Thresholds are determined from the variability of distance settings of two needles, one above the other, moving at constant and equal speeds back and forth across a field. Evidence for complex integrative action is manifest in several respects. Of particular interest is the marked dependence on illumination level, speed of movement, and size of field (30, 31).

The genetic development of space perception in the child is treated in a new text (32), and there are two attempts to assess the roles of maturation and learning in the depth orientations of animals (33, 34).

*Pulfrich phenomenon.*—There is fresh interest in utilizing the fact that a pendulum oscillating in a plane perpendicular to the line of regard appears to rotate out of its plane of oscillation if either eye is fitted with a filter cutting down its illumination, or if one eye is partially dark adapted prior to trials with free observation. The apparent displacement has been accounted for in terms of the difference in latent periods for the responses of the two eyes. The explanation seems especially attractive for Lit's data, taken at various illumination levels, if one adds the assumption that the visual latent period is inversely related to the loga-

rithm of the brightness (35). Observations indicate that latency decreases as the eye light adapts (36). Liang & Piéron's results, showing the chronostereoscopic effect with colored screens before one eye, may point to a new tool for research in color theory (37). One peculiar phenomenon, a marked increase in depth acuity when a dark glass is fitted over one eye, has been reported again (38). One may guess that the renewed interest in the Pulfrich phenomenon will lead to a series of publications on latency effects, for latency long has been an aspect of response that has interested the physiologists.

*Acuity.*—Important contributions to the measurement of acuity will be mentioned in the section on acuity testing. Data on the visibility of moving objects, a topic neglected heretofore, have appeared (39). The O'Brien's (40) have cast doubt on the thousandfold variation in sensitivity among foveal cones which Hecht postulated to explain the increase of visual acuity with brightness. Over a range of four log units for the surround brightness, the brief flash intensity necessary for discriminating two small adjacent test objects was never more than twice the illumination at which neither was visible; and the test objects were arranged so that most of the flux from each was concentrated in the area of three retinal cones. Thus the variation in sensitivity cannot approach the magnitude which Hecht's theory required. A juxtaposition of the points of view expounded by Hecht and by Crozier is presented in Sender's summary on the physiological basis for visual acuity (41).

*Ocular tremor.*—Any theory of the action of the retinal elements underlying acuity must posit some state of motion—zero or otherwise—for the eye during fixation. Methods for recording eye movements, one utilizing ultraviolet light reflected light from the cornea to a multiplier photocell (42, 43) and the other entailing photography of a beam reflected from a scleral mirror (44), the method of Adler & Fliegelman, have been described. Probably the photocell system is not sufficiently sensitive to yield information on the very fine oscillations which some theories have assumed. At a meeting of the Optical Society some inferences on the extent of any tremor were drawn from the nature of the perceptions of a single small detail seen in brief bursts of a train of 10 microsec. flashes. Because multiple images were not seen it was concluded (6) that any tremor must be less than 2 min. of arc. The facts on ocular movement should become established soon.

*Span.*—Gibson's results (45), discrediting the claimed benefits from training in reading digits and counting spots with flash presentation, have not been challenged, though research on the question is reported underway (46). Some interest in the increased span permitted by higher intensities and longer exposures has been shown. Especially where eye movements are effective, grouping effects in the stimulus play an important role (47). Saltzman & Garner (48) have repeated, in part, the Hunter-Sigler study of the relationship between exposure time and the number

of objects (dots or concentric rings) recognized. Furthermore, they have tried using voice reaction time for responding to the stimulus as an alternative to the tachistoscopic method. On the whole, the results appear similar. Incidentally, though the concept of span is useful as a rough description, there actually is no minimum number of objects below which the reaction time remains constant; slightly more time is required to perceive three rings than two, and more for two than for one. Schlosberg (49) has submitted a novel explanation for the Hunter-Sigler effect which fits the original data satisfyingly in the range below seven or eight dots. The probability for reporting any number of dots correctly by monocular vision is predicted simply by raising the probability for seeing one dot to the power indicated by the number. Thus if at any flash duration and brightness there is a 70 per cent probability for reporting one dot correctly, then there is a 49 per cent probability for two dots, 34 per cent for three, etc. A more exact test of the formulation is in press (50). The results support the monocular formulation but indicate that the binocular process is more complicated than assumed.

*Figural aftereffects and distortions of form and shape.*—The minimum distortion that can be detected in squares presented tachistoscopically has been determined. In contradistinction to the established dependence of intensity discrimination and acuity thresholds upon illumination level, shape distortion appears independent of changes in illumination intensity (51) over the range of intensities covered. It will be most remarkable if independence of intensity holds when the range of brightness is extended. If it does, then efforts should be made to trace out the bases of this peculiar stability.

The distortion of a straight line superposed on fields of radii or of concentric circles has been analyzed (52, 53) according to a formula derivable from early data on the variation in the position of the apparent vertical and horizontal as tilt is changed. The analysis has not gone without critical comment, however (54).

Material on figural aftereffects continues to accumulate. An illusion of apparent movement toward the observer, and of increasing size, is reported by Fernberger (55) as an aftereffect from watching receding patterns; as he states, James called attention to this third dimensional effect in his description of the waterfall illusion. A dramatic demonstration cited by Kohler as an instance of the figural aftereffect has been studied (56) with added controls, and the evidence for the operation of a figural aftereffect in the demonstration is equivocal. This particular demonstration utilized fixation on a circle, followed later by fixation upon two squares, one above the other, with the upper square falling where the circle had appeared before. The dimmer, smaller, and more distant character of the upper square, following the circle, is verified; but there is some evidence that with no prefixation upon a circle the same phenomenon may be reported. This query of the applicability of the Kohler-

Wallach hypothesis to a single demonstration seems like academic quibbling in comparison with Smith's broadside upon the satiational theory of the figural aftereffect. Very recently Smith (57) has pointed out that the assumed cortical projections of points on the two nasal retinae are such that the neural interaction postulated in Gestalt theory for the phi phenomenon cannot possibly take place. Yet one can sense phi with alternate stimulation of the two nasal areas (58). Now this account is followed by a discussion, wherein, after tracing the history of Kohler's development of the theory, Smith contends that the satiational hypothesis is not readily testable and that, because of certain objections, the entire theory should be abandoned (59). Andrews & Robinson (60) propose a decaying aftereffect from the arrowheads operating in opposition to waning traces from a stabilizing system derived from border effects, instructions to the subject, etc., as an explanation for the shift in the perception of the Muller-Lyer illusion with increasing time delays. Suggestions forthcoming from this approach to aftereffects may provide fresh data for a reworking of the satiational theory to permit exact predictions and tests.

In a series of papers Asch & Witkin (61 to 64) have explored the roles of sensory factors in creating the framework with reference to which objects are oriented and localized. Striking differences from individual to individual exist, but nevertheless some generalizations may be drawn from the investigations of body tilt, field tilt, and degree of articulation of the visual field. There is evidence for the primary importance of visual factors in the perception of the upright, with postural determinants secondary. The effect of the visual field upon the perceived upright becomes stronger and more consistent the more richly articulated the field.

The principles developed in psychophysical experiments in this area may be exploited for their implications for social psychology. Possibly psychophysics may soon find the traffic in ideas running in the opposite direction, for there is a growing literature on changes in perceptions under different stresses. Certainly the vision laboratories will begin to borrow from the social field for significant variables if the Postman-Bruner data (65) on adjustments of the size of a circle of light to match objects of different need or value are verified. They found the value of the object to the subject, as well as its physical size, to govern the adjustment.

*Reading.*—A large number of reports deal with the efficiency with which information may be grasped visually. Under this heading should be listed not only the papers treating the effects of print size and illumination level upon blink rate, comfort, speed, or efficiency of reading, but also the reports on arrangement of displays and evaluations of reading aids.

Because most applied research in these topics deals with special conditions and with specific instruments, there is a fair probability that

projects may produce results which a few years hence will be considered mere pot boiling. The value of any applied research must be judged with many factors in mind: urgency, generality, or possibly the number of years before the equipment to which it applies will be supplanted. Certainly, data that are useful now are being produced. As illustrative of work on dial design, we may cite Grether's comparisons of five different principles of instrument indication (66). A standard airspeed indicator, a rotating dial, a rotating pointer, a moving pointer on a linear scale, and a direct-reading counter are compared. Grether concludes that moving pointer instruments are superior to moving scale instruments for ease of check and qualitative reading. A series of papers (67 to 73) from Johns Hopkins presents some practical and timely recommendations for operation of radar with a particular kind of indication (the Plan Position Indicator). Experimental checks of the effect of screen brightness on the visibility of signal traces reveal, for instance, that an operator may increase radar ranges for weak echoes simply by turning up his screen brightness control (70). Operators tend to prefer a dim level, probably because a dark screen affords strong contrasts for the strong signals. Any such result which contributes greater safety and more latitude to commerce is well worth some research time. But it is distressing that such a finding could not have been calculated long ago with some general visibility functions and some transformation systems for them that would permit extension to display screens with their various time constants for rotation and decay of phosphorescence. Critical empirical checks to verify calculations will belong always in any vision operations analysis; but as the Hopkins series stands now, a large number of observations must be repeated to establish the empirical functions every time a new type of screen may be adopted. On the other hand, however desirous one may be for thoroughgoing systematic treatment in the applied fields, he should not overlook the value of the development of techniques for experimental tests. As a matter of fact, in one applied study comparing 80 telescope systems of 18 different designs for the range at which the telescopes afforded detection of objects from aboard ship (74), perhaps the most significant achievement was the technical design of the method for the comparisons. The ranges yielded were in good agreement with computations based on light transmission, magnification, exit pupil, and contrast rendition data for the telescopes and on the Tiffany Foundation monographs for visual contrast discrimination and range under various meteorological conditions (75). Unfortunately, the gain in technical knowledge may be lost, for the field study very likely will be forgotten except as an event which served to validate the theoretical computations.

Controversies over the optimal illumination level and the techniques for determining efficiency for reading printed matter remain lively (76 to 86). Speed is defended as an index of reading by Tinker's demonstration that speed of reading, with understanding, is reduced under ad-

verse conditions. The reduction was only 5 per cent when copy was vibrated to about the same extent as occurs when riding on a fast train (76) but the index did reflect the poorer condition reliably. Tinker points out that blink rate, a criterion employed by Luckiesh, is slightly lower for seven point newsprint than for the more legible 12 point bookprint (77). Nor is new evidence for the value of blink rate as a criterion of visual efficiency lacking (80). Although controversy over the method for determining reading efficiency has thrown into question the high illumination levels recommended by the Illumination Engineering Society, Luckiesh continues to defend them (81, 82). Additional relevant material may be found in Bitterman's discussion of the human effort factor (83, 84), Moon & Spencer's view (85) that with adequate surround vision will continue to improve indefinitely as level of illumination is raised, and Simonson & Brozek's work on performance and fatigue under different illuminants (86).

*Interpolation.*—An urgent problem today is how best to display air traffic on a screen. Data are appearing which should help with some of the questions. Details on the nature of the errors in estimating bearing angle have been reported by the Mt. Holyoke laboratory. For example, the length of a radial bearing indicator proves to have little effect on the accuracy of the subject's estimates of angle (87). In fact, with just the outer tip of the indicator showing, subjects are more accurate and less variable than when the indicator extends to the center of the display. Accuracy is greatest and variability least at the vertical and horizontal, and there is better performance also in the middle range, around 45 degrees. Anyone working on anchoring effects will find the Mt. Holyoke series interesting. Correlated studies of distance judgments on displays are under way (88). The accuracy in estimating the relative distance from a mark to the center of the screen by interpolation between concentric reference rings shows a relationship to the known positions of the rings which is analogous to the function for accuracy in estimating bearing angle with the vertical or horizontal. Incidentally, except for displays with very closely spaced reference rings, the size of the screen has no effect upon the accuracy with which one estimates a distance represented. In other words, Weber's law applies.

The enhanced accuracy and reduced variability for judgments of angle and distance at terminal boundaries and in the middle region are very striking. Possibly the anchors in such simple perceptions are too strong for the adaptation effects described by Helson (89) to come into play prominently. On the other hand, Helson's theory has been applied successfully to data on reports of magnitude. The theory fits the observed auction values in a card game for which all mathematical probabilities are known (90) and in his presentation Helson was able to demonstrate conformity with Philip's data on the distribution of absolute judgments of color mass (91). But both these situations involve interpolations with

XUM

boundaries or frames of reference of a different stability than in the interpolation tasks described above.

*Color theory.*—Evans' book (4) has been mentioned. Another book (92) emphasizes early history of color research. Some difficult syntheses to account for colors of objects of small subtense and, correspondingly, stimulating few receptor elements, lie ahead. That color theory is still in the jungle is attested by several recent essays (93 to 101). These articles are scattered over several topics. For example, there is Hartridge's outline for a new polychromatic theory (93), a brief summary of discussions of new and classical theories at the 1947 London conference (98), Pumphrey's statement, in the light of the work of Granit, Willmer & Hartline, on current opinion as to the evolution of color vision (99), and Hurvich & Jameson's historical note on shifts in Helmholtz's thinking with regard to simple three component or polychromatic theory (101). Arguments for using subjective colors to select the primaries have been advanced (102, 103), with Cohen & Gordon furnishing a well-documented account of the philosophy and history of the approach. Characteristics of negative after images are treated (104). Judd (105) has reviewed the literature on the color perceptions of the deuteranopic and protanopic observers and has been able to furnish quantitative estimates of their perceptions for the colors in the Munsell book. Sloan & Wollach have found a case of unilateral deuteranopia: a mild deuteranomaly in the left and a clear deuteranopia in the right eye. With his right eye blues and yellows are the only colors seen (106).

The course of chromatic adaptation has become a central issue in theory, and experimental data are appearing at a rapid rate (107 to 111). The disagreement in the findings of Cohen (107) and of Hartridge (108) on the question of whether adaptation to a narrow-band yellow is similar to that for a yellow created by combining two bands should be clarified. According to Cohen, two such yellows induce identical adaptation effects; but Hartridge says the monochromatic yellow turns to gray whereas the dichromatic does not. Brightness and field size may be extremely critical for adaptation phenomenon. Thomson (112), using 15 min. fields, has attempted to measure photopic brightness discrimination over several log units at various wave lengths. He reports that, at low levels, brightness discrimination is much better in the color regions presumed to produce the modulator activity postulated by Granit. It is assumed that at these low levels the dominator is less active and thus that the modulator response contributes more to the total brightness sensation: hence the minima in intensity discrimination. On the other hand, at high levels the sensation is presumed to depend mainly upon dominator activity, and the intensity discriminations show little dependence upon wave length.

The spacing of colors in different color systems has been studied (113) and the report of the Color Terms Subcommittee of the Inter-

society Color Council has been issued (114, 115). A plea for revision of the I. C. I. standard photopic luminosity curve, especially in the blue violet, appears in an article furnishing tables of relative spectral luminosity at luminance levels from scotopic to photopic (116). A careful redetermination of the scotopic visibility curve has been submitted (117).

*Light and dark adaptation.*—Effects of the state of adaptation upon detection of weak signals have been explored (69) and the course of change of sensitivity during illumination of the fovea has been established for different intensities (118). For the human dark adapted eye, sensitivity to short flashes is poor immediately after the onset of the adapting light, then improves to reach an optimum, after which there is a slight decline. The optimum is reached within 200 sec., even when adapting to 5,000 photons. The data show qualitative agreement with the older findings on light adaptation in Limulus, where single fiber discharges were studied. Considerable work must be done before the kinetics for such a process are understood, but some success in accounting for the oscillations in sensation during the first instants of exposure to light is reported (119).

*Successive flashes.*—The stability of the critical flicker frequency has been confirmed (120), and Brown & Gebhard (121) have commented at length on the sensations when a large uniform field, viewed monocularly, is made intermittent at a frequency below the critical flicker point. A study of these sensations for different parts of the spectrum has been taken as an approach to the primary colors (102). Sweet has reported a decline, as one goes out from the fovea, for temporal discrimination of two brief flashes appearing tangentially adjacent to each other (122). Data on pairs of flashes at threshold to complement the material on double flashes with Limulus (123) are lacking. In Limulus, the occurrence of a response to a first weak flash creates a bias against a response to an immediately succeeding flash. On the other hand, a subliminal first flash will increase the likelihood of response to a second flash occurring within the next second.

## Advances in Testing

*Visual acuity testing.*—Statistical techniques have been employed with some success in analyzing the interrelationships of the various tests for acuity. A factor analysis of the data for a large population examined on 14 wall charts (124) yielded factors which have been called retinal resolution, brightness discrimination, form (letter) perception, and simple form perception. Another population furnished measures both on acuity tests (including acuity scores taken with each of three popular testing machines) and on phoria tests, and these data similarly have been analyzed for common factors (124, 125). There is still ample room for clinical and analytical studies of testing procedures (126), but at least one question that seemed so elusive of solution has been cleared up. We

do not need separate measures of acuity for distant objects and for near objects if the subject has normal accommodative powers (125, 127). This is the traditional view, but it is interesting that it had not been substantiated well until someone applied the tools of statistics to data derived from machines designed, apparently, with the contrary view in mind. The popular testing machines furnish separate scores for near acuity and for far acuity. An acuity examination specific for some distance is probably the best screening procedure for selecting employees who must do most of their work at that particular distance (128, 129, 130); but one can hope that eventually we will go beyond the screening test level to make visual job analyses and studies of ocular function in various seeing tasks.

Statistical methods for deducing factor relationships are being applied in other testing fields besides acuities and phorias. Jones (131) has determined the intercorrelations of the sensitivities of 92 subjects at 20 spectral points, and reports that three factors account for the individual variabilities. Using the methods of partial correlation and multiple regression, Hirsch & Weymouth (132) have made additional calculations based on Stenström's measurements of axial length, corneal radius, depth of anterior chamber, and refractive state of 1,000 eyes. When axial length is held constant, corneal curvature shows a high correlation with refractive state. The future must test the fruitfulness of these sophisticated techniques, but the fact that they are being applied is certainly noteworthy.

The field of testing has a reputation for hard-boiled empiricism, with a study of graphs and tables preferred any day to the impressions and views of the specialist with experience. This attitude finally is pervading the field of eyesight examinations (133 to 139). Some basis for determining the proper standards for safety in piloting aircraft already has been laid (134). Attention was paid to the development of criteria for flight performance, and then groups representing different ranges of visual abilities have been checked against these criteria. Such work is tedious but valuable. The internal analysis of the statistics from tests may show us what we have tested, but the pedestrian empirical checks against performance criteria show us which tests we should retain.

*Color vision testing.*—Chapanis has published some informative comparisons of pseudo-isochromatic tests (140, 141) and two accounts describing exactly how to conduct quick mass screenings have appeared (142, 143). Bridgman reminds us of the merits of the anomaloscope as a supplementary test intermediate between the usual screening type and detailed laboratory studies (144). New developments are: Chapanis' promising attempt to construct a pseudo-isochromatic test which would grade people on a continuum instead of simply classifying them into Pass-Fail categories (145); Freeman's efforts toward a test independent of the color temperature of the illuminant (146, 147); and finally, Reed's

data on reaction time, suggesting that the speed of various color discriminations might be utilized as a testing device (148).

## Physiological Bases of Visual Functions

*Visual purple.*—By treating vitamin A aldehyde as well as vitamin A itself with strong acids, Ball *et al.* (149) report that narrow photosensitive bands are produced corresponding in spectral location with the color modulators described by Granit. Donner & Granit (150) argue against the "rod-cone" dichotomy. They propose instead that some asymmetrical chromophore of visual purple exists in the retina in several different states. The chromophore exists in a state dependent, in part, upon the receptor proteins "which from extreme rods to extreme cones at the other end of the scale well may differ considerably from one another." Wald & Hubbard have reviewed information on bleaching (151).

*Electroretinograms.*—Attempts to correlate the retinal response on the one hand with the discharge of nerve impulses (152) and on the other with psychophysical observations are going on. Some correspondence exists between changes in the retinogram and changes in certain psychophysical functions during dark adaptation. But one should not strain too hard to make electrical response curves representing the increase in response to a standard flash coincide with conventional threshold curves obtained by varying stimulus intensity (153). Riggs *et al.* (154) have found good comparability between the psychophysical data on spectral sensitivity and the intensities necessary at various wave lengths for producing a response of a given small voltage. This held at both photopic and scotopic levels. However, light of shorter wave lengths is somewhat more effective in arousing electrical responses than one might expect with a strict parallelism.

*Nerve impulses.*—Hartline has reported inhibition effects from adjacent retinal areas in the records from single fibers of Limulus (155). A large transient increase in frequency of firing occurs in response to a small increase of illumination. When the added illumination is removed, after being on for a long time, there is another burst of activity very nearly equal to the first (156). Granit and his co-workers are actively pursuing the variation in off/on ratios and polarization effects with particular interest in their significance for mediating color (157 to 160).

*Cerebral damage.*—Bender & Teuber have a wealth of clinical material on patients with occipital or occipitoparietal lesions (161, 162). Exaggerated form distortion, erroneous localization, reduced critical flicker frequency, and conspicuous impairment in the ability to perceive apparent movement are cited for patients with alterations in pattern vision. Some of the effects should be observable in decorticate animal preparations (163).

*Thresholds and quantal variation.*—Hecht's hypothesis that the source of variability is in the stimulus rather than in the organism

(164) has been employed to account for detection of signals by daylight vision (165). Bouman & van der Velden conclude from their distributions that only two quanta are needed by the rods of the dark adapted eye (166). This figure is, of course, radically lower than the five or more which Hecht had assumed from his distribution curves. Otero *et al.* find that absolute thresholds measured with various lenses interpolated before the eye show approximately a 50 per cent increase with the proper correction for nocturnal myopia (167). They question whether Hecht's apparatus, on which the quantum data on rod thresholds were collected, was designed to reckon with this factor. Some of the quantal theory has been utilized by Baumgardt for his treatment of the excitation process (168). There seems to be no argument that the energy needed at the retinal level is very small. But the extent to which the quantal hypothesis can account for psychophysical variation at threshold remains yet to be established (169).

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XUM

# HEARING[1]

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In reviewing the progress made during the past year in the field of hearing, it might be assumed that there is a clear starting point, that both the writer and his reader know what was the state of the field at the time the review is to begin. Such is probably not the case, for there has been no definitive summary of work over the past 11 years, while during this time, progress has been very rapid in the field. What seems like simple basic concepts now need a critical reformulation. To define the stimulus for hearing, for instance, has become a highly technical and complex job. In the absence of a clear-cut starting point, it becomes necessary to assume a considerable degree of sophistication on the part of the reader, not to mention implied agreement on a number of debatable points. With these reservations, we start our review. It may be hoped that the continuation of this series may lighten the task of future reviewers.

## Historical

Wise judgment can only be made by an informed intelligence. Far too many of the studies to be reviewed suffer from their lack of scholarship, from a lack of historical perspective, and, even more, of sound grasp of the neighboring fields of physics, anatomy, and neurophysiology. The day has passed when psychologists can view such naïveté with complacency.

A brief, yet pleasant excursion into the history of our knowledge of hearing is furnished by Békésy & Rosenblith (1) who have written about early theories about the ear and the processes of hearing. Their story stops with the end of the eighteenth century. Modern theories, and perhaps as much modern nonsense, seem to have been anticipated many times in the past.

Hirsh (2) has also added an historical chapter. His review covers binaural summation, or more properly binaural interaction, over the past century. Hirsh's review emphasizes again the keenness of observation which, in many of the older investigators, was coupled with crudeness in method and lack of experimental control.

## The Absolute Threshold for Hearing

Hardly any startlingly new facts have been discovered about the absolute threshold of hearing. Licklider (3) has recently summarized our

[1] This chapter was prepared with assistance from Contract N5 ori-76 between the U. S. Navy, Office of Naval Research and Harvard University, Project NR147-201.

best estimate of the absolute value of this function.[2] Nevertheless, the definitive study of the absolute threshold remains to be done. No single study adequately combines the necessary psychological, physical, and statistical controls. The course to be followed in such a definitive study is clearly indicated. First, as Pollack (4) has pointed out, we must decide where and how to measure the stimulus. A choice must be made among free-field, coupler, or probe-tube measurements. If secondary considerations dictate the choice of one of these measurements, data should be available for the conversion of one set of measurements into another. Happily, the necessary physical instruments are now available [see, for example, DiMattia & Wiener (5), Wiener & Ross (6), and Wiener (7)].

The second problem to be solved is the statistical one of sampling. The experience of the Bell Telephone Laboratory at the New York World's Fair makes it quite clear that a half million people can be tested and the resulting sample may still be seriously biased. During the past year Myers, Harris & Fowler (8) have reported audiometric tests done on nearly 1,500 war-veteran college students. Their results show the practicability of group audiometry, but, with more than half of the group passing the most difficult tests, adequate statistical analysis is hardly possible. Until a good deal more is known about the variability of normal hearing, any statement about the percentage of hard-of-hearing in the total population has no more usefulness than the entirely arbitrary limit which is set by each investigator for such "normal" sensitivity.

Of more psychological interest is the so-called "atonal interval" reported independently by Pollack (9) and Myers & Harris (10). The name has obviously been chosen to parallel the achromatic interval in vision, and it describes the fact that near the absolute threshold of hearing a listener cannot detect the tonal character of a sine wave stimulus. Just how intense the stimulus must be before it has a distinguishable pitch will depend on the criterion established by the individual listener. There is rough agreement that the tone must be from 2 to 5 db. more intense than the absolute threshold. This observation is related to the fact that pitch discrimination deteriorates rapidly as the intensity of the tone approaches threshold. The writers mentioned disagree in their estimates of the threshold for changes in pitch, but they agree that there is a five- to tenfold decrease in pitch discrimination in the 10 db. range above the absolute threshold.

Lüscher & Zwislocki (11) confirm the fact that the variability of the absolute threshold is useful in differentiating conductive and perceptive hearing losses. As first proposed by Békésy (12), the existence of recruitment in cases of perceptive loss should lead to a more rapid change of loudness near the threshold as well as in the range above it. This is reflected in turn by a lessened variability in the measurement of the threshold. For practical reasons, Lüscher & Zwislocki favor the use

[2] For a critical analysis of the factors which provide a theoretical basis of the absolute threshold, the reader should consult Segal (103).

of a measurement of differential sensitivity, by means of an amplitude-modulated tone, 40 db. above threshold in preference to the measurement of variability at the threshold. The results appear to be much the same. If so, this emphasizes the fact that the absolute threshold is in all likelihood indistinguishable by any functional tests from a differential threshold. The task of the listener at the absolute threshold is to distinguish between some residual noise level and a minimal level of activity just detectably greater.

### The Threshold for Repeated Short Tones

Three independent studies have been made in the last two years, each starting from an entirely different theoretical interest, of the threshold for short pulses of pure tone. Let us compare the experimental results. It should be pointed out, first of all, that short bursts of a pure tone are no longer "pure" at all; they have become highly complex physical stimuli. Ten cycles of a 1,000 c.p.s. tone, coming in bursts repeated once a second, will have only 1 per cent of the total energy of a steady tone of the same amplitude, and, of this 1 per cent, 99 per cent will be in frequencies other than 1,000 c.p.s. leaving 1 per cent of the 1 per cent as the "pure" tone.

To facilitate a comparison of the three studies the writer has recalculated the results so that they can be compared directly. These values are plotted in Fig. 1. The data from Garner (13) are taken without change. These results show the amplitude of a tone during a pulse plotted as a function of the duration of the pulse. The curve has a negative slope, that is, as the pulse becomes shorter in duration the amplitude must be greater. Garner's results are referred to the reference pressure of 0.0002 dyne per sq. cm., which may be taken as approximating the steady-state threshold. De Vries' (14) results required some slight revision. They were recalculated using the more customary 50 per cent point of the psychophysical curve in place of de Vries' extrapolated 0 per cent. Finally, the data of Pumphrey & Gold (15), plotted originally against the number of cycles per pulse, have been regrouped in terms of the duration of the pulse, the results for the four frequencies 800, 1,200, and 2,500 c.p.s. averaged, and the mean intensity change plotted against mean duration. The data of de Vries and Pumphrey & Gold can presumably be referred to the long time thresholds of their listeners.

When plotted in this way the results are as remarkable for their agreement as for their disagreements. All three curves have about the same slope over at least part of their course, and this slope suggests a first-order reciprocal relation of time to intensity. On the other hand, the displacement of the curves along the abscissa is quite inexplicable. There is, in the accounts given, no ready explanation why de Vries' results suggest a time constant fully 40 times as long as that of Pumphrey & Gold. One comment might be added. Doughty & Garner (16) report that tones shorter than 10 msec. in duration have no pitch. Presumably, then, all of the stimuli used by de Vries sounded like tones, none used by Pumphrey &

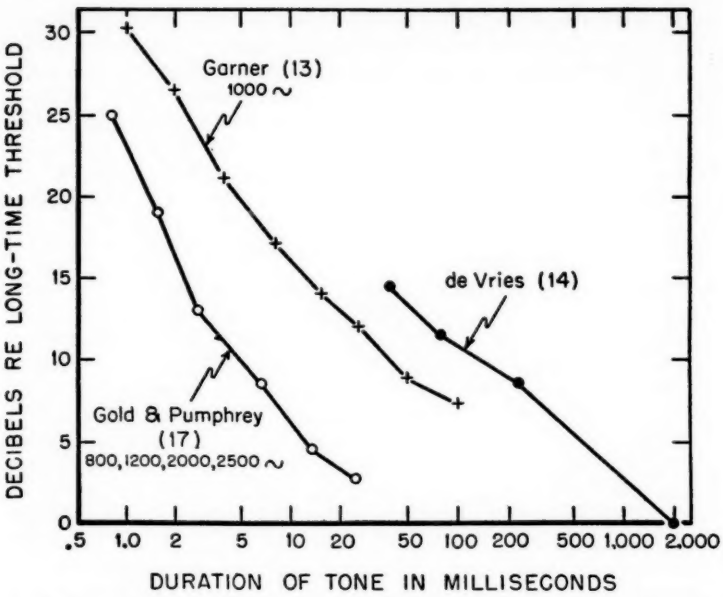


FIG. 1.—ABSOLUTE THRESHOLD FOR TONES OF SHORT DURATION COMPARED WITH CONVENTIONAL THRESHOLD FOR STEADY TONES. THE ORDINATE REPRESENTS PEAK AMPLITUDE DURING THE SHORT PULSE.

Gold did, and Garner's were split either way. Perhaps none of the listeners was perfectly clear what it was he heard.

## ENERGETICS OF THE THRESHOLD

In the short space available here, it will not be possible to do justice to the theoretical issues raised by the authors of these three studies. It is unfair, however, to let the matter go without brief comment. Gold & Pumphrey (17) base their argument on a calculation using the dissipation constant $Q$, so familiar to electrical engineers. They make these assumptions: that the ear contains a series of independent resonators, that there are more of these resonators than the number of discriminable pitches, and that the threshold of hearing is determined by the amplitude of vibration in the most sensitive resonator acting alone.[3] It is then possible to calculate from their data a dissipation factor, or time constant, at each frequency. Their results shown in Fig. 1 give at 1,000 c.p.s., for example,

[3] It is fair to add that $Q$ is useful only so long as we are dealing with a simple, linear circuit, so that the use of this concept carries the implied assumption that the ear is linear over its range of response and that all elements of mass, stiffness, and friction may be treated as lumped constants.

a $Q=50$, or a time constant $t=16$ msec. Were Garner's experimental results subjected to the same treatment, we would get a $Q=150$ and a $t=50$ msec. These values represent a very lightly damped system and are completely at variance with other findings. Békésy's (18) measurement of the damping of the cochlear partition shows, converted to the present terms, a $Q=2.2$ which is independent of frequency. It should be added that Gold (19) himself recognizes certain physical considerations which make his line of reasoning untenable unless supported by other hypotheses. To explain why so little friction is encountered by the resonating elements moving in the fluid of the cochlea, he supposes that the cochlear microphonics furnish a positive feed-back.

In the writer's opinion, the most serious fallacy in the argument of Gold & Pumphrey lies in their assumption of closely spaced, independent resonators and, as a consequence, in the neglect of the more than 99 per cent of the energy contained in the "side-bands." The nature of these considerations is best understood by reference to Garner's paper (20) which is reviewed below.

The series of papers by de Vries review the very general question of the minimal energy necessary for stimulation of a sense organ. American readers are familiar with the case for "quantal energy" in vision, as it has been worked out by Hecht (100). Sivian & White (21) raised the related question in hearing some years ago when they pointed to the thermal agitation of molecules in the air as a theoretical limit to the acuity of hearing. De Vries (22, 23) concludes, after reviewing this argument, that Brownian movement in the air is not a limiting factor. On the other hand, he estimates that the energy levels which are produced at the sense cells within the cochlea by threshold stimulation may come much closer to the energy supplied by Brownian movement. De Vries believes that it is at this point, rather than at the eardrum, that quantal considerations set the absolute threshold.

As in the previous case, de Vries' estimates of energy levels and rates of energy flow depend critically upon a number of assumptions about the structures involved. And once again we come up against the hard fact that our actual knowledge of the working of the cochlea is so limited that any line of argument is highly speculative. It is interesting, of course, to guess that the experimental constants may be of this very small order of magnitude; but what shall we assume to be an independent system within the cochlea? What happens to these energetic calculations when two systems are more or less closely coupled together? Which of our measurements depends on physical processes in the ear, which on an unknown process of stimulation of the sense cells, and which on properties of the primary nerve cells? Without answers to these questions the argument is not only speculative but also idle.

### Short Tones and Bursts of Noise

The ability, given by the judicious use of vacuum tubes, to produce and control short samples of an auditory stimulus has borne fruit in other

directions. At least six studies, other than the papers mentioned above, have been reported during the past year which made use of abbreviated sounds. Henry (24), for instance, has reported on what were, stimulus-wise, differential judgments of the duration of a tone. He reports the Weber ratios for seven durations between 32 and 480 msec. and again for three durations, 47, 77, 277 msec., and for four intensities, 20, 40, 60, and 80 db. sensation level. Since there is a reciprocal relation of time to intensity within this range of durations, it is not surprising that the values obtained are closely predicted on the assumption that the basic discrimination is one of differential intensity rather than one of duration.

The pitch of short tones was determined in a thorough study by Doughty & Garner (25), re-examining a question on which there has been conflicting evidence. A listener does not find the judgment an easy one, and it becomes progressively more difficult as the duration of the tone falls below 0.1 sec. Any consistent shift in pitch is a function of the method, of the intensity, and of the frequency of the tone tested. Doughty & Garner conclude that the shift depends on the complex spectra which result when short tones are produced, the listener judging on the geometric mean of a tonal mass rather than on the basis of a single salient frequency.

Of far more interest are Garner's results (20) on the loudness of such short tones. When a train of short tones is given, the average energy of the train is naturally proportional to the percentage of the time during which the tone is turned on; but the loudness turns out to be a complex function of the time fraction and of the rate of repetition, as is shown in Fig. 2 taken from Garner's paper. Attention is called to the four points joined by the dotted line in the lower right part of this figure. These four points all represent tones which have been turned on for one-half of the time. Each of them has been equated in loudness to an 80 db. steady tone. The equivalent intensity of the interrupted tone ranges from 77 to 81 db. for the four rates of repetition. Evidently, equal energy does not insure equal loudness. As a matter of fact, the interrupted tone is more effective than the steady tone. In these four cases, equal average energy would have been provided if the interrupted tone reached a level of 83 db. during each pulse. Actually, not 3 db. more intensity, but 3 db. less during each pulse was required.

The line of argument will, from our discussion above, be reasonably clear. The problem is not one of time relations so much as one of energy distribution within the acoustic spectrum. Pulsed tones do not represent a single frequency, but a band of frequencies. These results argue that the same total energy spread out in a number of frequencies is far more effective than when it is concentrated in a single frequency. It remains to be said by how much and with what set of conditions are the interrupted tones more effective. As a first step in this analysis, the writer has replotted in Fig. 3 the data of Fig. 2 to show the extra effectiveness of the interrupted tone relative to a constant level of average or long-time energy. The results are plotted as a function of the time fraction, since this repre-

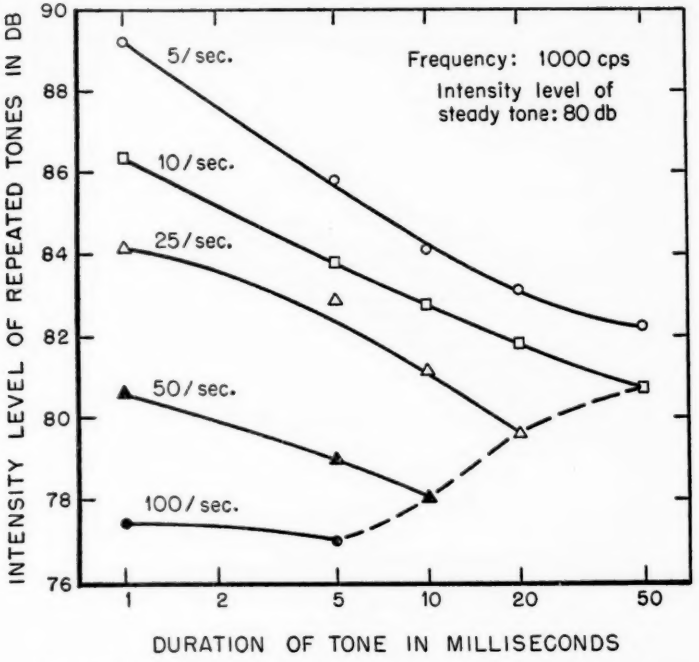


FIG. 2.—PEAK AMPLITUDE OF REPEATED PULSES OF TONE WHICH ARE JUDGED EQUAL IN LOUDNESS TO A STEADY TONE OF 80 DB. RESULTS ARE PLOTTED FOR FIVE RATES OF REPETITION. THE DOTTED LINE JOINS POINTS REPRESENTING 50 PER CENT TIME FRACTION. FROM GARNER (13).

sents a constant shape of the modulating pulse. The greater effectiveness of the short pulses of tone reaches impressive proportions, 12 to 14 db., with the short-time fractions. It is evident, however, that the data are still not plotted against the proper parameter. Neither the pulse duration nor the time fraction bring the four curves together. Almost certainly some parameter is required which describes the envelope of the acoustic spectrum.

The underlying theory was stated several years ago by Fletcher & Munson (26), and it is amply substantiated in this study. The argument is simply that energy which is distributed among a number of discrete frequencies acts independently to produce separate components of loudness. The total loudness depends upon the sum of the individual components; and, since loudness is not a linear function of intensity, the total loudness is not proportional to the energy. The soundness of this reasoning is attested by computations which Garner reports based on the formulae of Fletcher & Munson. The agreement between calculated and ob-

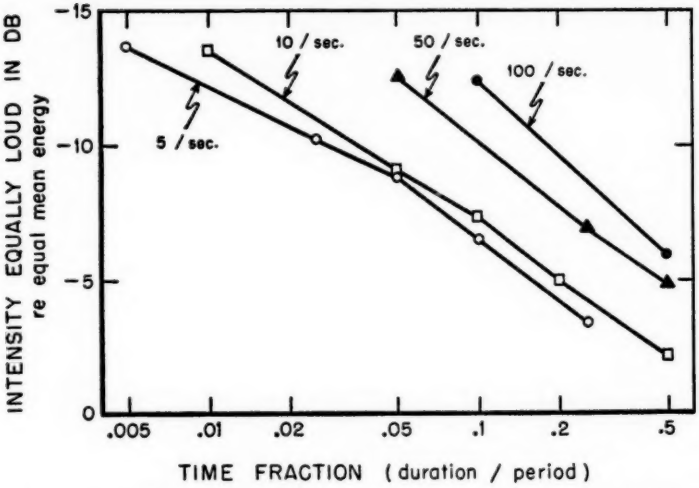


FIG. 3.—ADVANTAGE OF INTERRUPTED TONE OVER STEADY TONE WHEN RESULTS OF FIGURE 2 ARE PLOTTED IN TERMS OF AVERAGE LONG-TIME ENERGY. ABSCISSA HAS BEEN CONVERTED TO TIME-FRACTION, BUT NOTE THAT NEITHER THIS SCALE NOR PLOT AGAINST ORIGINAL ABSCISSA BRING THE FOUR FUNCTIONS INTO COINCIDENCE.

served values is reasonably good. Furthermore, by an extension of this argument, Garner believes that the threshold data discussed above can be better explained in terms of a summation of subliminal "loudness" than in terms of the storage of physical energy per se. One is reminded of Békésy's conclusion (18, 27) that physical factors alone will not explain the rate of decay of loudness; both the long decay of sensation and the summation over a long time at threshold suggests "storage" in the sensory cells or in neural elements.

As an antidote for any overconfidence from which the reader may suffer at this point, it may be well to turn to two comparable studies using short bursts of noise by Miller (28) and Miller & Taylor (29). In the first of these, Miller reports on the apparent loudness of short bursts of noise. He reports that, once the duration has fallen below a critical value, shorter bursts of noise become weaker, but at a rate slightly less than would be predicted on the basis of their loss in energy. The slope of the functions below this limit vary between 0.8 and 1.0. More important is the fact that the critical minimum duration is much shorter at high than at low intensities. This means that intense noises reach their final loudness in a much shorter time than do weak noises. Miller estimates that a noise requires only 60 msec. to reach a 90 db. sensation level, and 140 msec. to reach a 20 db. level. Thus, we see that the threshold results for noise are quite comparable with those for tones, i.e., there is a rough reciprocity of

time and intensity. But at supraliminal intensities, the results differ markedly. Wide band noise continues to show the reciprocal relation while tones certainly do not. Conversely, short bursts of noise give clear evidence of a relatively slow growth in loudness, while the results with tones do not. There is one striking difference in the procedure in the two experiments that should be noted: Garner used pulses of tone repeated at a moderately high rate, while Miller compared only single bursts of noise. It is still conceivable that part of the "excess" loudness of the pulsed tones is to be explained in terms of nervous facilitation which results from the critical timing of intermittent stimulation.

Miller & Taylor (29) experimented with what they call auditory "flutter," the experience produced by rapidly interrupting a continuous sound. It will be evident at once to the mathematically sophisticated, that a rapidly interrupted white noise presents an interesting problem. Interrupting a tone, it will be recalled, produces a different distribution of energy in the auditory spectrum than does a steady tone. On the other hand, interrupting a noise which contains all frequencies at the outset cannot change the distribution of energy; it can only limit systematically the phase relations of the various frequency components present. Quite evidently, if flutter is perceived this must be accomplished without the benefit of Helmholtzian resonators.

In listening to flutter, three important stages are to be distinguished at rates below the point where complete fusion takes place: at very slow rates the listener hears a succession of bursts; at a slightly higher rate the noise acquires a distinct pitch which gradually rises; and at still higher rate it passes over into a noise with little or no pitch, but differing in quality from the continuous noise. The boundaries between these stages are a function, not only of the frequency of interruption, but also of the intensity and of the sound-time fraction. The range for the second stage, over which there is an impression of a definite pitch, runs roughly from 40 to 250 interruptions per sec. Good listeners were able to match the noise to a pure tone about as well as they could a train of pulses or square waves, these latter stimuli with a single salient frequency. Further, their differential sensitivity for changes in the rate of flutter corresponded closely with their differential sensitivity for the pitch of low tones. The results lend support to the view that periodicity of response underlies the discrimination of tones with frequencies below 250 cycles.

One additional study by Miller & Garner (30) completes this cycle of papers. This is a study of the masking afforded by repeated bursts of noise. After an interesting analysis of the data, the authors are able to show that the effectiveness of the masking (measured by the ratio of masking with continuous noise to masking with interrupted noise) depends on the duration of the silent interval. The longer the "window" through which one can listen in, the greater is the ability to hear a tone. Furthermore, corresponding closely to the results of Doughty & Garner on threshold for tonality, it requires a longer "look" to hear a low pitch than a high pitch.

Lüscher & Zwislocki (31) have reported additional measurements of what they choose to call "adaptation" of the ear. There is little question that they are dealing with the same mechanism which produces masking. There is an experimental difference in the fact that they measure the shift in threshold, not simultaneously, but by interposing a very brief test tone at various intervals after the end of the masking tone. Typical curves show that the threshold returns to normal in 0.25 sec. The single curve published suggests that this may be followed by a short supernormal phase. The correspondence of "adaptation" to masking is quite great. Above 15 or 20 db., both masking and adaptation increase as a linear function of intensity. Moreover, curves which show the spread of adaptation to other frequencies than that of the adapting tone closely resemble the familiar masking curves of Wegel & Lane (101).

## Loudness, Pitch, and Volume

Work on the more conventional aspects of psychophysics has produced no very startling results during the past few years. Pollack (32) reports that the presence of noise as a background reduces the apparent loudness of a sample of speech which is heard against that noise. We have here a kind of simultaneous contrast. Pollack is able to express his results in terms of a subtraction by a given level of noise of a certain number of loudness units from the speech signal. Obviously, the effect is greatest when the speech is just audible above the noise. Pollack (33) also comments on the ability of listeners to judge the relative loudness of various kinds of noise ranging from a low rumble to a high-pitched hiss. Actually, this experiment tried to answer the question whether his listeners could make judgments of volume, of annoyance, of density, and of force, which were independent of judgments of loudness. Volume, density, and annoyance seem to be distinguishable from loudness; force probably meant loudness to these listeners. Thomas (34) has published a rather thorough study of equal-volume contours, first reported some years ago by Stevens. The present results are given in absolute terms, i.e., in terms of sound pressure, rather than in relative terms. Furthermore, Thomas has pushed the contours well out toward the edge of the auditory area. Underlying questions about the source or nature of the judgment of volume remain unanswered. It does seem clear, however, that volume is not a mediated judgment, which would rest in turn on loudness and pitch.

The discussion of absolute pitch continues on its indecisive way. Bachem (35) has presented curves to show that the change with frequency of chroma, the musical counterpart of pitch, stops with spectacular suddenness at about 4,000 c.p.s. According to this view a tone of 20,000 c.p.s. is indistinguishable from one of 4,000 c.p.s.; but 4,000 c.p.s. is immediately distinguished from, let us say, 3,900 c.p.s. Happily, this limit of chroma falls almost precisely at the upper limit of the piano keyboard. Results such as this are difficult to explain, yet it may be that there is a unique property of tones, best called chroma, which is not heard by most people. The problem remains how the 0.1 per cent of the population who hear chroma are going to explain it to the rest who do not.

A few brief notes on methodology: Harris (36) reports studies pointing to the importance of absolute judgment in measures of pitch discrimination, and he proposes (37) certain test procedures to minimize this factor. Tresselt (38) remains convinced that a time-error can be produced in judgments of pitch, even though she agrees with Postman (39) that it is not normally present. Your reviewer finds this argument most confusing. First, we find that there is no time-error, and then we manage to produce errors where none grew before. What does this mean? This whole field badly needs some clearer terminology and a few explicit hypotheses. It would be interesting to see, for instance, how far the suggestions of Helson (40, 41) or of Michels & Helson (42) about adaptation level could be extended into the field of hearing. A start in this direction is that made by Johnson (43) who talks about "scale construction" in pitch. The range of Johnson's stimuli was so great, that his predictions, based on a concept of the "category threshold," show only rough, but reassuring, agreement with his data. Johnson's category threshold is apparently equivalent to Helson's adaptation level.

### Masking and Differential Sensitivity

The standard studies on the differential threshold for pitch and for loudness were done, most naturally, under optimal conditions of quiet. These studies showed a loss of sensitivity as the tones to be discriminated approached the absolute threshold. It has remained for later and as yet somewhat unsystematic studies to show that one effect of masking is to create, at higher intensity levels, a situation not unlike that present at the absolute threshold. These later studies were mainly done during the war and had, therefore, a very practical formulation. Thus, an aviator's ability to follow a radio range depends upon his making an intensity discrimination of the range signal heard above a background of considerable noise. Discrimination deteriorates rapidly as the ratio of the signal to the noise becomes unfavorable. This basic fact is clear from a host of such studies. A start on the more systematic investigation of these effects is provided by two studies reported by Harris (44, 45) on pitch discrimination. He has obtained results for a 1,000 c.p.s. tone at one masking level, and fragmentary results for two lower frequencies. Any interpretation of these results will have to wait for more complete studies.

### Interaction of the Two Ears

For many years it has been a familiar experimental device to switch from one eye to the other in order to isolate, presumably, peripheral from central sensory phenonema. Furthermore, several sorts of stimulus interaction have been studied by making use of these two independent pathways into the nervous system. Curiously, the two ears have rarely been regarded in the same way. Their only function has seemed to be sound localization. Renewed interest in possible interaction now reveals a series of effects, many of them, to be sure, more laboratory curiosities than anything else, which, taken together, may afford a valuable tool in understanding the whole auditory mechanism.

There are some ways in which the two ears are obviously different from the two eyes. When looking with the eyes, it is impossible to say without some external check which eye is being employed. On the other hand, a listener usually knows at once when one ear alone is stimulated. There is, thus, some independence in their reporting to the cortex. At the same time, there are a few clearly peripheral effects. One ear often differs markedly from the other in sensitivity. There may be some diplacusis which is presumably peripheral in origin. Masking appears to be completely independent in each ear, and when cross-masking takes place, it is a result of cross-conduction, probably [Békésy (46)] through the air around the head.

In what way, then, do the ears interact? Our first interest was a practical one. In testing equipment, we needed to take thresholds in the open air. Were these thresholds influenced by the use of two ears as opposed to one? Shaw, Newman & Hirsh (47) and Keys (48) reported that there was modest summation at threshold, about 3 db. if the two ears were equally stimulated with tone; and the even smaller figure of 1.9 db. has been reported by Pollack (49) for noise. These are interesting, but hardly impressive results, and they may prove [Smith & Licklider (50)] to be little more than the statistical advantage of having two hooks in the water to catch the wandering fish. It was rather surprising, therefore, to have Hirsh (51) report that when thresholds were measured above a substantial masking noise, something quite different happens. There now appears to be substantial interaural inhibition. Two ears, it turns out, are not only not better than one, they are actually worse. With a 250 c.p.s. tone, the disadvantage may amount to as much as 8 db.

The explanation of the Hirsh phenomenon is not evident at once. Why should a mechanism which at one level gives summation at another level produce subtraction? At least this is not some kind of built-in inhibition as we would find in reciprocal innervation. One observation can be made which may suggest the direction of an explanation. This is the fact that the monaural tone always sounds clearly at one ear while the (binaural) noise lies between the ears. Put more strictly, the nervous system has available in the monaural tone an additional bit of information about it which is lacking when the listener hears the binaural tone. This information is derived from the fact that the messages reporting the tone come in over only one trunk while the messages about the noise are coming more or less correlated over two.

This review of the matter leads directly to the further observation reported by Hirsh (52) with reference to the relative phase of noise and of the tone in the two ears. In its simplest form, the observation is this: the inhibition which has been reported occurs when both noise and tone have the same phase relation between the two ears. This is true whether the signals have the normal, synchronous polarity or whether both tone and noise are reversed in polarity so that pressure in one ear occurs at the same moment that rarefaction occurs in the other. But when either noise alone or when the tone alone has reversed polarity, there is once again

summation! These facts, in all their detail, are rather confusing, but the reader can keep the matter fairly straight by remembering that inhibition occurs when the tone and the masking background noise have the same apparent localization, and that summation occurs when they are phenomenally separated.

That these observations are not of purely academic interest is shown by Licklider's study (53) of the masking of speech rather than of pure tones. He used the same phase relation as did Hirsh in the last experiment. Licklider added a third condition describing another possible relation of the noise in the two ears. This was a random relation in which there was no correlation between the pressure in the two ears. The rank order of the result was the same: with the speech in phase, the highest scores were made when the noise was out of phase, intermediate scores were made with random phase, and the poorest scores with the noise also in phase. Reverse the polarity of the speech in one ear, and the above order is exactly reversed. As a final word, it should be added that the uncorrelated noise was localized even more clearly in each ear than was the noise with reverse polarity. This case, then, provided an exception to the ready generalization in terms of the apparent localization of the sound. As a result, Licklider leans to an explanation for all of these effects in terms of some form of central masking.

Before leaving this somewhat confusing subject, two other papers deserve brief mention. First, Hirsh & Pollack (54) tested the effects of noise on the apparent loudness of tone above the masked threshold. This was done under the same two "homophasic" and two "antiphasic" conditions used by Hirsh in the threshold study. The result showed that there were closely parallel effects on loudness within roughly 20 db. of the masked threshold. Another curious variation of what is probably the same underlying effect has been reported by Egan (55). In this case, speech comes to one ear of the listener, noise to the other. The effect of turning on the noise is to make the speech sound louder than it did in the quiet period. Were the effects not so small, we might believe that it is possible to hear on the telephone in a subway station.

Diplacusis, a difference in the pitch experienced when each of the two ears is stimulated with the same frequency, has recently been given renewed attention because cases have been reported with changes of nearly half an octave when one ear is temporarily deafened by a very loud tone, and because changes of as much as 5 per cent have been measured in normal ears at higher frequencies. In two recent papers, Jeffress (56, 57) has shown that there are similar, if less spectacular, effects at much lower frequencies. His listeners matched in pitch not only the usual two modes of presentation, i.e., R-ear alone (monotic) and L-ear alone (monotic), but they also compared each of the monotic conditions with the two ears together (diotic). Finally, they compared two dichotic conditions in which there was a phase difference between the ears and hence a difference in localization.

In general, the amount of pitch change is relatively small, averaging

perhaps less than one per cent. The more marked deviations involve comparisons with one or both of the monotic tones, but it is not possible to account for the results in terms of a difference between the two ears alone. In some cases, the diotic tone differs from the average pitch of the monotic tones by an amount which is much greater than the diplacusis for the two monotic tones. On this basis, Jeffress argues for a central or "interaction" factor in the perception of pitch in addition to peripheral factors (such as place of stimulation in the cochlea), a view which is plausible for the low frequencies (300 c.p.s.) with which he is working.

The results obtained when both ears are stimulated with a phase difference between them are less impressive. Nevertheless, for some people a tone localized in the right ear sounds different in pitch from one localized at the left. Curiously, when using a population of different subjects and frequencies in the first study, there appears to be a negative correlation between the monotic diplacusis and the dichotic difference in pitch. In the second study, using a population of day-to-day changes with a single subject, the correlation is positive. Inconsistencies of this order make it difficult to see the full implications of Jeffress' experiments at this time.

## Hearing of Speech

During the past year, a report by Egan (58) on the articulation testing methods employed by the Harvard Psycho-Acoustic Laboratory makes this material generally available. Since these procedures are already well known from other sources, they need not be discussed here.

Two additional studies of the intelligibility of distorted speech have been reported recently. In one of these, Pollack (59) reports the results of removing successive bands of either high frequencies or low frequencies from the speech signal. Intelligibility was determined by having listeners write down what word they understood over a particular system. The procedure followed closely that used by French & Steinberg (102) some years ago, and the present study is a valuable supplement to their but recently reported results. The difference between the two experiments, important in view of the use to which the data have been put, lies in the fact that Pollack's experiments were run against a substantial background of noise. When properly evaluated, this information should improve the accuracy of computational methods such as those suggested by French & Steinberg for predicting the intelligibility of speech under conditions of stress.

Licklider, Bindra & Pollack (60) and Licklider & Pollack (61) report the results of experiments on "infinite" speech clipping. An electronic device is employed which turns the speech waves into a series of square pulses, the pulse turned on or off each time the original speech wave crosses the 0 axis. The loss in the intelligibility of the speech is very much less than might be expected, and it is actually of little consequence if the higher frequencies in the speech wave are emphasized before the clipping takes place. This extends Licklider's earlier argument that the effects

of various kinds of distortion are quite unequal. From the present experiment, it is quite evident that the amplitude of the individual speech wave is not an important variable for its understanding. Licklider, Bindra & Pollack (60) present interesting three dimensional, time-intensity-frequency plots showing the effect of two types of distortion and suggesting the direction in which further analysis should be made.

## Sound Localization

Among the interesting studies on the effects of bodily rotation being conducted at the School of Aviation Medicine, Pensacola, is one describing the illusory displacement of a sound following rapid deceleration. Clark & Graybiel (62) rotated subjects in a Bárány chair, stopped them suddenly, and then asked at 5 sec. intervals for reports on the apparent localization of a sound placed in various positions in front of the listener. With the particular accelerative stimulus used, there is a quite substantial displacement of the sound from its real position, amounting to about 15° in a direction opposite to that of the subjective rotation. This illusory displacement is rapidly dissipated, the localization returning to near normal in about 30 sec. The writer has analyzed that data presented by Clark & Graybiel and finds that the effect shows an almost perfect logarithmic decrement. The displacement can be represented by an equation

$$d = e^{(a-rt)}$$

in which $d$ represents angular displacement, $e$ is the base of natural logarithms, $a$ is a constant reflecting the initial amplitude, $r$ is a constant related to the rate of recovery, and $t$ is the time from the start of deceleration. For the data of Clark & Graybiel's Table III, $a = 2.6$ and $r = 0.0945$. This may be put another way by saying that the time constant of the logarithmic decrement is almost exactly 10 sec.

It has become increasingly evident in recent years that the whole story of sound localization is not told in terms of the binaural factors in simplified or static situations. Wallach, Newman & Rosenzweig (63) report experiments defining what they call the "precedence effect." Two sounds arriving from different directions, such as a sudden noise and its echo, fuse into one impression if the interval between their arrivals is sufficiently short. The localization of the fused sound is determined, however, principally by the localization of the first sound to arrive. The second sound has some small effect, but even this is significant only when the second sound is close to the direction of the first. The same results were produced synthetically by a series of four clicks in proper sequence to the two ears. There are a number of practical situations in which the precedence effect is of importance. Perhaps the most common is a reverberant room in which each sound echoes many times yet is heard in its proper location. The placement of speakers in public address work and the engineering of stereophonic sound systems offer other instances. Unpublished experiments by Rosenzweig suggest that there is a fairly direct neural correlate of the precedence effect in terms of the refractory

phase of the higher auditory centers. This point, however, needs further clarification.

### Deleterious Effects of Sound

Although the problem is hardly a psychological one, there continues to be a lively interest, which often borders on anxiety, about the harmful effect of this or that form of stimulation at sonic and near-sonic frequencies. The lack of adequate measurements and authentic evidence makes it well-nigh impossible to give clear answers to the various questions which have been raised. Even where there are data about the physical properties of the sound, there may still be inadequate information about the manner in which the sound is applied to the human organism and the consequent degree of coupling between source and the organism. Lippert (64) has published a comprehensive graph which facilitates the intercomparison of various measurements of sound and vibration. Davis (65) has emphasized the necessity for more information, especially in the ultrasonic range. Thus far, the evidence indicates that the most serious effects of ultrasonic sound are produced by familiar mechanisms, such as burning or overheating, and that the damage which results might be more readily produced by the same energy supplied by almost any other method than the sonic one.

### Mechanical Properties of the Ear

It is simple, and convenient, to think of the ear as consisting of a diaphragm, the eardrum, which is connected by a simple mechanical link, the ossicles, to a physico-physiological transformer, the cochlea, which transforms mechanical energy into the appropriate nerve messages which are sent to the brain. Each of these elements tends, in this simplification, to assume a nearly ideal form, is maximally efficient, and is, at the same time, loosely coupled to the succeeding element. The fact that this description is only very roughly true, and that the last two requirements are physically incompatible, disturbs us very little. The same thing is true of the early models of the cochlea. It was regarded in "either-or" terms: we accepted either a place theory of pitch or a frequency theory of pitch.

The factual information on which a more sophisticated account of the ear's functioning can be based is accumulating rapidly. Unfortunately, an adequate treatment of this subject is quite beyond the scope of the present review. The reader must refer to the original papers of Békésy (18, 66 to 69), Wever & Lawrence (70), Wever, Lawrence & Smith (71, 72), Rasmussen (73), Kobrak (74, 75), and others for details. Summary accounts have been given by Wever (76) and Békésy (77). For the benefit of the reader, let it be said simply that the ear would appear to be a fairly closely coupled mechanical system in which the principal elements of mass (inertance) are furnished by the ossicles and the fluid of the cochlea; important elements of stiffness (elastance) by the round window membrane, by the suspension of the stapes, and by the volume of air in the middle ear; while friction (resistance) is almost certainly

distributed at a number of points throughout the system. The mechanical analysis of sound by the cochlea is apparently accomplished independently of the overall mechanical properties of the ear. It depends upon a hydrodynamic rather than on a simple mechanical resonance. Furthermore, the importance of each of the elements in the ear varies from one individual to another as well as from one species of animal to the next. Caution is indicated in making generalizations from one animal to another.[4]

### Neurophysiological Aspects of Hearing

In contrast with the important contributions made recently on the physical side, publication of significant work on the physiological aspects of the sensory processes in the ear has lagged far behind. There is no lack of work in progress, but the loss of headway as a result of the war has not yet been made up. Moreover, the complexities in the neurophysiological picture are so great that caution is required in interpreting even the most limited findings.

In a brief note, Galambos & Davis (78) report evidence which suggests that what they formerly called activity of single nerve fibers (presumably fibers of the first-order neurons) was in reality potentials from the cell bodies of the second-order neurons belonging to the cochlear nucleus. Most of the functional relations described in their earlier reports are quite unaffected by this new interpretation. The methodological implications are, however, of considerable significance, for it is probably true that with our present techniques, it is physically impossible to record the action potential of a single sensory nerve fiber *in situ*.

Wever & Lawrence (79) have carried out a new series of measurements of the stimulus intensity-cochlear response relationship in the guinea pig. The new curves follow in general form the earlier ones of Wever & Bray (80) and of Covell & Black (81) on cats. The remarkable feature of all these curves has always been the extremely precise linear relationship which they show over a very wide range, at the same time that every other measurable aspect of the cochlear response shows variability of an order more typical of a physiological mechanism. In presenting a conceptual picture of how these results may come about, Wever & Lawrence make use of the notion of a pattern of stimulation in the cochlea which varies as a function of the frequency of the stimulus. Space does not permit a discussion of this ingenious account, nor of the many puzzling questions which there still are about the nature and possible functions of the cochlear microphonics. It is hoped that this area may be explored more fully in another review of this series.

Finally, two other lines of physiological work should have brief mention. Walzl (82) has summarized some of the more important studies in which responses have been recorded from the auditory cortex. The reader should be warned that there are still very large gaps in our knowledge of

[4] A recent and quite ambitious attempt to give in exact mathematical terms a "Theorie der Schneckenmechanik" is contained in a thesis of Zwislocki (104) which reached this reviewer too late for inclusion above.

the whole auditory system. For instance, there are two important differences between central and peripheral recording. First, most of the peripheral work has been done with continuous tones, and most of the analysis is meaningful only in terms of steady state responses. The cortical work depends almost entirely on the stimulation of the ear with clicks or short bursts of tone. Secondly, the large and important component of all cortical responses is the slow wave of either positive or negative polarity. Fast waves, on the other hand, are found both at the cortex and in peripheral neurons. The relation of the fast to the slow waves is at present obscure.

Toxic effects of drugs on the inner ear have been known in some measure for a long time. Two new studies add something to this picture. Hawkins (83) has reported on rather severe disturbances in vestibular functions in rabbits and cats as a result of the administration of streptomycin. He does not mention disturbances of hearing, but these may be present. More serious effects were produced by Juul & Vraa-Jensen (84) who gave ascaridole to guinea pigs. This drug, sometimes used as a vermifuge, produced degeneration of the hair cells in the organ of Corti. This degeneration was accompanied both by a loss of functional hearing (as tested by simple reflexes) and a corresponding loss in cochlear potentials.

## Deafness

For the first time in many years, psychologists and lay readers have available an authoritative treatment of deafness in the recent book by Davis (105) entitled *Hearing and Deafness*. Recent clinical publications have been concentrated very largely in two areas, improved methods of diagnosis centering around a variety of audiometric techniques, and studies of traumatic losses. In neither area are the findings of great importance in an understanding of normal hearing, however useful they may be to the clinician. Improved electronic hearing aids are gradually winning very wide acceptance, an important social gain. At the same time, the more spectacular treatment of deafness remains the fenestration operation, and its most famous proponent is still J. Lempert of New York. It is regrettable that there is not more information of a scientific nature available about this operation. There are a few exceptions. Thurlow, Silverman, Davis & Walsh (85, 86, 87) report the results of a variety of auditory tests before and after fenestration on two series totaling 216 patients. These studies show the usefulness of statistical control in evaluating new departures in operative technique; but there has been reported almost no work of an anatomical or physiological nature which helps to clarify the *modus operandi* of the *fenestra novovalis*. Rather, we are presented with certain theoretical speculations (88) about hydrodynamics by the originator of a variant technique of fenestration, speculations of truly incomparable confusion.

The critical problem in diagnosis is the distinction between conductive and nerve deafness. It is supposed, for instance, that fenestration may benefit the patient to the extent that he suffers from a conductive loss.

Lewy, Shapiro & Leshin (89) have reviewed the recent progress in testing hearing. Recent suggestions apropos of diagnosis include the computation of a Social Adequacy Index for Hearing by Davis (90), loading of the external meatus with water or mercury by Johansen (91), application of excess pressure to the ear by Rasmussen (73), observation of the stapedius reflex by Kobrak (92), the use of cochlear potentials by Lempert, Wever & Lawrence (93), and various methods of estimating recruitment by Békésy (12, 94, 95), by Lüscher & Zwislocki (11), and by Dix, Hallpike & Hood (96). Several of these suggestions are of much more than passing interest, but space does not permit their discussion.

The interest in traumatic losses is one of long standing. It is partly a theoretical interest, but has been given recent impetus by clinical material arising in the war. This recent material has been summarized by Perlman (97) whose articles also report experimental results on a series of rabbits exposed to revolver blasts. Perlman correlates the results of functional tests of hearing, using the tensor reflex, with histological examination of the ears. He finds in some cases a profound loss of function with only very limited end-organ damage as seen by the microscope. In all but one case, the damage was limited to the inner ear with no observable effects on the middle ear. Chronic preparations usually showed greater changes than did acute ones because the former allowed time for degeneration to take place after the initial injury. There is a gross correlation of damage and loss of hearing, but the detailed losses do not bear any close relation to observed injury. In this, the findings agree with the results of Smith & Wever (98) and Smith (99), who used pure tones rather than transient shock waves. There obviously should be a close relation of damage to loss of function. Apparently, our present techniques, particularly those of histological examination, are quite inadequate to reveal the basis of a functional loss.

## Summary

After a survey of the work on hearing such as we have just concluded, there are four points which stand out quite clearly.

(*a*) Progress has been rapid in those fields which have benefited from the great technical improvements in acoustics in recent years. Good examples are the systematic use of noise as a stimulus or of recorded samples of speech. In some cases, this has meant that old problems have been explored with greater precision; in other cases, quite novel problems have been opened to attack.

(*b*) These new techniques have forced experimenters to become more expert in related fields. It is no longer possible to do work on hearing without a substantial understanding of the physics of sound; and this requires, in turn, at least a moderate comprehension of mathematics. On the other hand, the rapid increase in physiological work has been hindered by the lack of available knowledge of anatomy. An excellent illustration of the interdisciplinary ramifications of a single problem is furnished by the recent work using pulsed tones.

(*c*) Perhaps, because of our preoccupation with new and exciting problems, there has been an almost total neglect of some of the other aspects of hearing.

(*d*) The basic theoretical issues of hearing, that is, the construction of an account of the empirical psychophysical correlations in systematic, analytic terms, are still unsolved. It is clear, on the one hand, that the older theories, such as resonance theories or telephone theories, are almost certainly wrong in detail. There is neither a tuned string nor a linear transducer in the cochlea. On the other hand, each of these older theories, if it is stated in the most general terms, is partly correct. Thus, we feel sure there is some degree of mechanical analysis in the inner ear. The only trouble is that such very general formulations don't help much in finding the correct details.

Much the same can be said for theories about the central nervous system. It is evident that there must be a correspondence between the formal properties of reportable, psychological processes and some function in the central nervous system. But just at the moment, speculation about what this hypothetical function is seems to help us very little. Every experiment which is done reveals the nervous system as an extremely intricate mechanism. What is needed is not more speculation, but rather more powerful experimental tools. Only experimental work based on detailed working hypotheses will lay the firm foundation for future psycho-physiological work.

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# SOMESTHESIS AND THE CHEMICAL SENSES[1]

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The year just concluded was both a poor one and a good one for the "minor" senses—poor in that the amount of original work published was distressingly meager; good in that some few publications in these fields represent outstanding scientific excellence. Of a bibliography of about threescore titles, more than half, as might be expected, fall within the somesthetic realm. Of these about half deal with clinical efforts to cope with the ever-challenging problem of pain. The chemical senses seem to have received something more than the characteristic neglect that has been their lot in scientific history. Five years ago, writing in Glasser's *Medical Physics*, Bienfang (56) said of odor that it "is unique among sense perceptions . . . it receives little attention, is universally apologized for when it is talked about, and most frequently is attributed to the sense of taste." Whether current interest in the field is anything more than a flurry is perhaps too early to determine. At any rate olfactory and gustatory problems are involved in about a third of the literature to be discussed, an unusually high proportion in any such accounting.

## Somesthesis

Since there is no universal organizing principle at work in the field of somesthetic research, it is not easy to decide upon a set of categories to aid in bringing order into what is intrinsically a chaotic literature. However, the following rubics somewhat satisfactorily accommodate the recent output: Basic Mechanisms, Neural Correlates, Clinical Techniques, Abnormal Sensitivities, and (inevitably!) Miscellaneous.

*Basic mechanisms.*—A careful investigation into the fundamental conditions of the production of superficial cutaneous pain has been conducted by Bishop (1). His experiment is not so much original in conception—in many respects it follows the thought of von Frey's early (1896) work on pain and comes to much the same conclusion—as it supplies a technique of experimentation that has much to commend it. Bishop employed tiny drops of solder impaled on needle points to provide a series of graded stimulators 0.2 mm. to 1.5 mm. in diameter. With such stimuli, threshold appears to be reached, irrespective of stimulus diameter, when the lower hemispherical surface has just been pressed into complete contact with the surface of the skin. The formula covering all cases is $g/r^2 = K$. It may be inferred from the general relation that the constant factor in pain arousal is lateral stretch of cutaneous tissue. Bishop carries the inference further; he believes that "lengthwise stretch of nerve terminals appears to be the effective stimulus for pain endings in mechanical distortion of

[1] This review covers the period from April, 1948, to July, 1949.

the skin." Several common facts of pain sensitivity lend credence to this conclusion. Assuming there to be no inflammation present, any sharp cut through the epithelium (if blood has not been drawn) will prove painful when the skin encompassing it is stretched. But immobilization of the tissue with collodion will prevent the occurrence of pain under the same general circumstances. The well-known surgical phenomenon of pain accompanying stretching of internal organs whereas cutting of the same tissues may not be an effective precipitant of pain is also cited as evidence supporting the general conclusion that lateral stretch of subepithelial nerve net is the necessary condition for pain.

Much the most spectacular of the year's experiments in the somesthetic field is that reported by Pattle & Weddell (2). Many students of cutaneous sensibility have become minor martyrs in their effort to learn more of what goes on under the skin. A number of investigators of the return of sensitivity following nerve section, several concerned with excision experiments (looking for the histological correlates of "spot" sensitivity), and some who have progressively sliced away tissue to determine somesthetic "end points" have all undergone at least minor surgery, and no little inconvenience, in the interest of their science. Weddell has now performed an experiment of the same bold cast and, although it is not as conclusive as one might wish, it has yielded important and interesting data.

The experiment was concerned with the arousal of pain through direct electrical stimulation of an exposed sensory nerve. The skin over the anterior and lateral side of the index finger was anesthetized and an appropriate incision laid bare the intact digital nerve. Sheet rubber was slipped between the nerve and the contiguous tissue and a pair of silver-silver chloride electrodes was hooked about the nerve. The skin was then sutured in such a way as to insulate the nerve and its attached electrodes from all tissue.

The preparation being readied, electrical stimulation was cautiously applied. Steady D.C. potentials were delivered to the nerve as were inductorium and condenser shocks. Double shocks were produced with the aid of a Keith Lucas pendulum. With a 6,000 ohm resistor in series with the nerve a direct current rheobase of 7.5 volts was found. At threshold the result in sensation was always the same, a pain "greatly resembling a wasp sting" being felt as localized in the finger pad (which was otherwise anesthetic to direct cutaneous stimulation). Time between the application of the electrical stimulus and any resulting sensation was measured by an oscilloscope, the stimulus triggering the sweep and a subject's key deflecting the spot.

Condenser shocks being gradually increased in strength by a graded series of capacitors, no sensory result was produced until a value of 0.1 mfd. (through a series resistor of 6,000 ohms) was reached. Then came the "wasp sting." The same quality of sensation and, moreover, with the same latency ($1.27 \pm 0.03$ sec.) resulted from all condenser values up to

6.0 mfd. A 7.0 mfd. condenser discharge produced a sharp break in the series for now "a long-lasting, severe, aching pain, which was completely different in quality from the 'wasp sting' reaction" resulted. The latency remained substantially unaltered, however (1.27 ±0.06 sec.). There was little repetition of the high intensity (7.0 mfd.) experiment. The aching pain was "not well tolerated by the subject" and it could not be systematically investigated.

Of special theoretical interest is the consequence of multiple stimulation. Repetitive shocks of supraliminal strength, if repeated within 0.1 sec., brought forth a sharp pain of the wasp sting variety which was felt as about twice as strong as a single shock of the same strength. If the shocks were separated in time by more than 0.1 sec., two distinct pains were felt. Within the limits of the conditions provided by the experiment there appeared to be no summation of subliminal stimuli. "When a just subthreshold shock was repeated six times within 0.5 sec. *no sensation whatever was aroused.*" This conclusion, if it obtains in general, is a most important one as bearing on certain of the classical theories of pain. It is a great misfortune that the authors did not arrange a much wider set of stimulus conditions. Higher frequencies and durations of shocks, whether square wave or sinusoidal, were definitely indicated. Not only were the possibilities present of settling some old arguments but the response of such a preparation to faradic, as well as galvanic and transient, currents would have been most interesting. Had the stimulating circuit been so designed as to provide a continuous determination of tissue impedance, capacitative and resistive, the quantitative aspects of the results would also have been more valuable. Apparently a curious transition in the character of the pain occurs quite abruptly at one point in the intensive series. One wonders whether it really is a "point." (The authors, by the way, make no claim that it is.) Definition of the stimulus at the electrodes with tissue resistance changes compensated would provide the only possibility of obtaining a clearly unambiguous answer.

One other paper dealing with electrical stimulation, but with intact skin, appeared during the year (3). The effort was to measure electrically-aroused thresholds of tactile sensation and of pain. Both square waves from a Grass generator, which would produce frequencies from 20 cycles to 500 cycles, and sinusoidal waves (20 to 2,000 cycles) were used. Frequency was said by the authors not to be an important variable and, throughout most of the measurements, a frequency of 100 cycles was used. However, by their own charted results the variation with frequency was perhaps the most interesting outcome. A plot of voltage (r.m.s. volts) against frequency (c.p.s.), over the range 20 to 3,000 cycles, reveals a function sharply concave to the frequency scale with a maximum sensitivity in the region of 1,700 cycles. Thresholds expressed in current units (ma.) show a steady rise throughout the frequency range. Power units are not plotted though they could be computed, of course, from the data presented. As in the previously cited study there was no attempt

either to measure or correct for tissue impedance changes which omission, except for the crudest clinical purposes, renders the measures relatively valueless.

As early as 1796 Darwin suggested that an inquiry into the effects of "whirling a person" at the end of a beam might have interesting effects on the symptoms of disease, notably headache. Considering the antiquity of the proposal there has been little attention paid to the possible therapeutic value of gravitational forces, though there have been ample opportunities for the observation of transient headaches produced in pilots by the sudden application of negative *g*. Kunkle, Lund & Maher (4), suspecting that headaches of vascular origin could be alleviated by centrifugal forces applied in the head to seat direction, have experimented in the human centrifuge with exposures to positive *g* forces. Headaches induced experimentally by distention of cranial arteries (histamine and caffein withdrawal) were entirely eliminated during the application of 2 to 3 *g*. Clinical headaches of vascular origin (from hunger, head trauma, and emotional tension) were similarly relieved by as little as 2 *g*. That the mechanism subject to such forces is entirely vascular is attested by another series of experiments in which pain thresholds at the forehead, determined by the Hardy radiant heat technique, were not significantly affected by centrifugal forces of as much as 4 *g*. Nor were headaches of nonvascular origin, produced by compression or irritation of the surface tissues of the head, notably altered by positive *g*.

Two papers of a speculative nature, concerned with the mechanism of pain, have appeared. Soresi (5) raises the question of whether pain is a "physiological" or a "pathological" sensation. He finds for the latter, believing that no specific end organ, free or encapsulated, exists for the sole purpose of eliciting pain. What we term pain is ". . . a complex, confused, unpleasant sensation . . . elicited by various nerve structures the biophysical insulation of which has become inadequate to secure the proper direction of nerve impulses . . . ." Pain apparently arises from afferent impulses that have run amok or have otherwise got out of bounds, producing chaotic disturbances in brain centers. The idea is reminiscent of Marston's hedonic theory (57) of some years ago (and other more worthy theoretical forebears, no doubt).

The other bit of speculation has to do with the localization of deep pain and has something more of sophistication about it. Harman (6) has collected a series of observations on referred pains in phantom limbs and has performed experiments on novocain block. From the mechanism in accordance with which the phantom limb sufferer projects his sensation into an anatomical unit he no longer possesses, the author argues that "deep" pains are normally localized in a "phantom" that corresponds with the real body. Localization of deep pain is a function of the cortex; it "is determined as much or more by the anatomy of the cortex as by the structure of the limb or part of the body in which it seems to be." Deep pain must always be an alloaesthesia. The pain of angina oftentimes is

localized in the arm whereas an ache from a knuckle is in the knuckle. If we "knew our viscera," localization of deep pain might be as relatively exact as with pain of peripheral origin.

The only recent contribution to the literature of basic somesthetic mechanisms which has the cast of a traditional psychophysical experiment is that of Harper & Stevens (7, 8). The work is an attempt to develop a psychological scale of weight or, more properly, a scale of intensity in kinesthesis. In hearing we have the sone scale of loudness and the mel scale of pitch. Psychological scales of visual rate and visual "numerousness" have also been developed. Now, as a result of these experiments, we have the "veg" scale of weight, the logic of its development being the same as that employed in the auditory and visual scales and based on general considerations previously set forth by Stevens (9). Veg, by the way, is a term developed from the root of an old Norse word meaning "to lift." Corresponding to each physical weight that can be lifted is a psychological weight which can be expressed in number of vegs.

Twelve subjects, in the Harper & Stevens experiment, were required to lift a standard weight, then select from a number of comparison weights one which was judged to be "half as heavy." The raw data show that, on the average, a value almost exactly two-thirds that of the standard was chosen as feeling "half" as heavy. By suitable manipulations of the data an empirical formula relating subjective weight to physical weight was derived. It has the form (where $V$ = number of vegs, $W$ = weight in grams): $\log V = 14.58 \log (1 + \log W) - 6.94$. The scale was anchored by arbitrarily calling the weight experienced in lifting 100 gm. 1 veg.

There are other, and more general, implications from this experiment. Earlier, Stevens & Volkmann (10) stated the principle that

> all j.n.d. based on a physiological process involving a change in the general location of excitation (or possibly a change in its form of distribution) will be found to be subjectively equal in size. All j.n.d. produced by a process of adding excitation to excitation already present will be found to increase in subjective size with increased magnitude of the stimulus.

Replotting Oberlin's data on lifted weights and bringing them into relation with the present results leads to the conclusion that the second of the two principles stated above obtains in this instance, that sensory judgments based on added stimulation are involved.

*Neural correlates.*—Positive progress may be reported in the effort to seek out the neural bases for qualitative variations in pain sensibility. Weddell and his co-workers (11) have made intensive investigations of the nature of the underlying tissue in instances in which pain of an especially unpleasant quality had been reported. In 59 cases wherein, as the result of cutaneous scars or partially denervated skin, pain variously identifiable as "protopathic," "paradoxic," "hyperpathic," "over-reaction," etc., had occurred, biopsies were taken after thorough sensory

testing of the affected areas. The authors' conclusions are unmistakable. In every instance wherein hyperalgesia could be elicited by a needle prick the underlying nerve nets and terminals were found to be "isolated from their neighbors." Conversely, in no case in which such isolation of terminals could not be found microscopically could "unpleasant" pain be aroused. The authors are of the definite belief that qualitative differences in pain are not to be explained in terms of morphological differences in pain endings, though they think that alterations in intensive thresholds might be brought about by the presence of growth cones on nerve tips.

The Weddell, Sinclair & Feindel paper (11) is also of value in that it reproduces some excellent photographs of single fibers and single terminals showing (*a*) interweaving nerve nets in normal skin, (*b*) isolated terminals from reinnervation tissue, and (*c*) recently denervated tissue undergoing invasion from neighboring nerve fibers. Still another of its values is the relation of the findings to some of the classical problems coming out of the regeneration studies of Trotter & Davies, Head, Lanier, and others. All trustworthy results of the earlier studies the present authors believe to be consistent with their interpretation of "sparse innervation" for "wicked" pain, as Head called it.

A contribution to the same classic problem of sensitivity accompanying nerve regeneration has recently been made by Sherman, Tigay, Arieff & Schiller (12). The contribution is a bit peripheral in that it concerns a comparative study involving return of sensitivity following experimentally produced lesions in the cat's sciatic nerve. However, the points of comparison with the human case are many and, moreover, the study is especially welcome in view of the paucity of literature on sensitivity changes during nerve regeneration in animals. Rabbits appear to have been the only previous animal subjects for such study.

Using withdrawal behavior to compression of the pad of the posterior paw and crying, shivering, or leg withdrawal to pin prick as indicators, the authors were able to plot the entire sensory supply of the sciatic nerve. The potential nerve overlap for the sciatic was also outlined. A large return through overlap occurs, as in man. Also, as in the human case, motor recovery may be well advanced before there is any evidence of sensory return. Pressure pain also returns before sensitivity to deep pin prick is restored.

Not the least of the contributions made by the authors of this study is their attempt to settle upon some suitable definitions of terms. The literature of this problem has been plagued by semantic difficulties. They propose definitions for: "anatomic sensory supply," "area of dual innervation" (intermediate zone), "residual supply of the nerve," and "area of isolated supply." One can hope that at least some of the terminological confusion may be abolished by this effort.

A conclusion reached by Lewis, Pickering & Rothschild (58), in their 1931 study of sensory changes accompanying the application of a pressure cuff to the arm, was that "sensory nerve fibres become more sensitive to

ischemia as they are traced back from their endings toward the central nervous system." Now a repetition, with some further elaborations of technique, by Sinclair (13) denies the generality of this finding. In point of fact Sinclair gets no such progressive change in all subjects. Moreover, a cuff applied to different portions of the arm results in inconstant mechanical effects, producing greater compression in the upper arm because of the structure of the underlying tissue. In the course of the study an incidental finding was that tactual sensitivity (stimulation of the skin directly) is more susceptible to ischemia than is "hair" sensitivity.

The Johns Hopkins work on central projections of peripheral stimulation is continuing and new studies have been reported on patterns of tactile representation in the thalamus of the cat and monkey, and in the cortex of the rat (14, 15, 16). The thalamic projection in the cat (15) is of special interest in that the contralateral surface of the body is represented in the responsive area (nearly the entire pars externa and pars arcuata of the ventral nuclear group) as a figure of the animal, with the tail pointed anterolaterally, face and mouth medially, and extremities inferiorly. Since it has been established (in the monkey) that the cervical segments are reversed as a group in cortical representation, it is of note that no such reversal has occurred by the time the thalamus is reached. This type of representation would also seem to preclude the possibility that separate topographical arrangements exist in the thalamus for the several somesthetic modalities.

New cases of dissociation of pain and temperature in cord lesions have been reported by Sherman & Arieff (17). Three cases show varying separations of pain and temperature, these perhaps requiring explanation in terms of separate pathways for these modalities. The fourth would be difficult to explain by any theory. As a result of an intramedullary lesion there was a loss of pain and cold with full preservation of warmth! As the author concludes, "The problem . . . is still not completely understood."

An anatomical study possibly having significance for interpretation of kinesthetic phenomena was reported by Rexed & Therman (18). The tibialis anticus and gastrocnemius muscles in cats were deafferented or deefferented and the number and size of sensory and motor myelinated nerve fibers to these muscles were determined. The flexor nerves had relatively larger numbers of sensory fibers than did the extensor. The sensory nerves varied from 1 to 17 micra and their distribution was trimodal.

*Clinical techniques.*—Chief interest in the clinical literature of somesthesis centers around the phenomenon of "extinction" (19, 20, 21). The effect in question was introduced to the field of clinical testing by Bender in 1945 (22). Certain patients with parietal lesions he found were unable to perceive two stimuli applied simultaneously to corresponding body points. They felt only one contact, that on the normal side. However, if single stimulation were employed no lack of perception occurred at any testing point. The normal sense process apparently suppresses the "weakened" one. This extinction phenomenon has been the subject of consider-

able investigation and has been found to obtain in vision, all the somesthetic modalities, and (less certainly) in audition. The present papers report (*a*) a new case of Alzheimer's disease demonstrating the phenomenon in touch, kinesthesis, vision, and audition (19), (*b*) a new case with partial surgical excision of the right frontoparietotemporal cortex who showed extinction, partial or total, in pain, touch, warm sensitivity, weight discrimination, two point discrimination, stereognosis, and texture discrimination (21), and (*c*) an attempt at relating the chief phenomena of extinction to theory (20). As might be expected, in view of the nature of the effects, the authors find the Gestalt mode of thinking most congenial and there is specific appeal to the theoretical constructs of Goldstein, Koffka, and Köhler.

The Hardy-Wolff-Goodell apparatus for measuring pain thresholds to radiant heat on the forehead has been used by Schilling & Musser (23) on 71 patients diagnosed psychoneurotic and 138 non-neurotic patients and medical personnel. If the "pain perception threshold" is defined as the lowest intensity for producing a "sharp, stinging, prickling sensation" and the "pain reaction threshold" as the lowest level at which the subject "winces or withdraws" it is found that no distinction between neurotics and non-neurotics can be based on the pain perception threshold. However, the pain reaction threshold distinguishes the two groups. The neurotics had a lowered reaction threshold with a mean of 0.447 gm. cal. per sec. per sq. cm. as contrasted with a mean of 0.502 gm. cal. per sec. per sq. cm. for the non-neurotics. The difference was significant with a CR of 4.9. The authors conclude that the technique "is of limited value in confirming the diagnosis of psychoneurosis but of no aid in ruling out such a diagnosis."

A simple clinical device introduced by Keele (24), a "pain chart" on which a patient records periodically his ratings of degree of pain experienced (slight, moderate, severe, agonizing), is remarkable only for the response it aroused on the part of his critics ("letters to *Lancet*"). Taking his cue from Virginia Woolf—"let a sufferer try to describe a pain in his head to a doctor, and language runs at once dry"—the four-point rating scale was devised with the hope that such a chart might be useful in the same way that a temperature chart is useful. The chart's "validity" seems to have been demonstrated, at least in part, by the fact that it checks with previous knowledge of the course of postoperative pain in the types of cases studied.

The remaining recent clinical literature is not notable. Lewey (25) points out the value of ascertaining the "number of spots" per unit area to supplement threshold measures in clinical diagnosis, certainly not an entirely novel suggestion. Goldblatt (26) reports lowered vibratory sensitivity, as measured by the Roth "neurometer," in certain thyroid and venereal disease cases. Dorcus & Kirkner (27) report the successful use of hypnosis in suppression of intractable pain. Posthypnotic suggestion was

especially effective in five of eight cases of chronic menstrual pain, whereas it was ineffective in five male cases of pain from spinal cord injuries.

*Abnormal sensitivities.*—An unusual case of total inability to experience pain is vividly reported by Boyd & Nie (28). The case is that of a seven year old girl who displays multiple scars, bruises, self-mutilation (but without any apparent masochistic pleasure), fracture dislocations, and local deformities. In the course of play she constantly skins knuckles and wrists and develops sores on her hands and knees. She has frequently burned herself on the coal stove without discomfort. "On several occasions the parents smelled odor of burning flesh and found her leaning casually on a hot stove." Cold produces the only source of genuine discomfort. Two hypotheses are suggested by the authors. The anomaly may represent a congenital structural defect with incomplete connections in the postcentral area with inability to "organize the complex concept of pain," a view that will certainly not appeal to the thalamic theorists. The other hypothesis relates the phenomenon to the mechanism of sensory aphasia. The possibility that both ideas may be utilized is not overlooked by the authors.

A case similar to the one described above has been reported to the reviewer by Hebb[2] as constituting the subject of an extensive series of cutaneous tests by McMurray (29). The only discoverable anomaly present was total lack of pain sensitivity. It is hoped that early publication will make the results available.

An unusual case of cold hypersensitivity is the subject of a paper by Mullinger & Bogoch (30). In most instances antihistamine treatment is expected to allay the symptom, the theory being that cold releases histamine or a histamine-like substance. In the case reported, several antihistamine treatments failed to produce the desired desensitization.

Glossodynia (burning sensations of the tongue) has been dealt with in a comprehensive review (31) of a wide variety of clinical conditions in which this symptom appears. In addition to avitaminosis B there are several hematologic, local irritant, neurogenic, and idiopathic causes.

*Miscellaneous.*—Attention should be drawn to two somewhat general treatises on somesthetic subjects appearing during the year. One is Wolff & Wolf's little book on *Pain* (32) which attempts a summary of the clinical and experimental literature of this subject. As might be expected the radiant heat technique for measurement of pain thresholds provides the central theme, though the treatment is semisystematic. The other is Jenkins' chapter (33) on "Studying the Skin Senses," appearing in Andrews' *Methods of Psychology.* The chapter deals rather intensively with stimulation methods and includes a selected bibliography of 55 titles.

Three papers concerned with apparatus of possible interest to the experimenter in somesthesis have appeared. Mauro & Black (34) describe

[2] Personal communication.

an electronic controller of volleys in electrical stimulation. A wide range of timed pulses and intervals between them is provided by a circuit having three main components employing gating, univibrator, and pacemaker stages. Schmitt (35) describes a radio frequency coupled tissue stimulator. This device avoids the common difficulty that, whereas it is now possible electronically to produce a stimulus of any wave form, amplitude, and timing, there has been less success in isolating such a stimulus, capacitatively and conductively, from ground. Schmitt's circuit accomplishes the desired isolation by means of a radio frequency link, modulation, and subsequent demodulation. The possibilities of practical usefulness are unlimited for all kinds of tissue stimulation, from single fiber work to more gross cutaneous application. A monograph by Andrews (36) on psychological apparatus provides a classified bibliography which contains a section on "Cutaneous Senses" and one on "Other Senses." Of interest to the somesthetic researcher are 47 titles, 22 of which belong to the chemical senses and labyrinthine phenomena.

One paper dealing with the affective tone of tactual impressions is contained in the year's literature (37). Fifty subjects, equally divided between the sexes, ranked 15 kinds of fabrics as to pleasantness and unpleasantness "by feel." Smoothness of texture made for pleasantness, in general, while roughness usually produced unpleasantness. However, regularity of pattern, even with raised fibers, made for pleasantness. A repetition of judgments after six weeks revealed fairly high reliabilities. There were no important sex differences.

## The Chemical Senses

*Olfaction.*—An old, even "classical" problem, that of the determination of the differential intensitive threshold in olfaction, has been worked over by Wenzel (38) with some technical improvements in apparatus and procedure. Using a modification of the "blast injection" technique, with temperature, pressure, and volume of the odorous stimulus controlled, four subjects were tested to obtain the Weber fraction. A single odorous compound, phenyl ethyl alcohol, was used. Since this substance has a calculable vapor pressure it was possible to express stimulus intensity (by use of Avogadro's number) in terms of number of odorous molecules.

The Weber fraction (84 per cent threshold) was computed to be about 0.15, a considerably smaller value than any previously reported. Individual differences were not notable. One "experienced" subject, an employee of an aromatics company, did not differ significantly from the others.

While there is, in the Wenzel study, impeccable statistical treatment at all steps and generally improved control over previous blast injection studies, there is, nevertheless, one serious source of disturbance in the technique. Stimulus intensity variations are brought about by manipulation of the pressure behind the blast. This is, of course, the basic trouble with the blast injection technique. Pressure variations, correlated with

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odorous intensity, provide a cue to judgments in any psychophysical experiment. Wenzel controls for this, however, running a series of odorless pressures and calling only for pressure strength judgments. Double plots of $p$ values against pressures reveal the two functions to be indistinguishable. Yet the author, because the subjects "felt that they were disregarding the pressure while judging odor," though admitting that "satisfactory demonstration of this impression at the behavioral level remains to be provided," concludes that the correct differential thresholds for phenyl ethyl alcohol have been obtained. It should be added that the author says that the chief advance made by the research is one of technique: "The results reported in this paper are thought to be more consistent with developing theory concerning the fundamental nature of olfaction than earlier reports since a molecular unit has been used to express stimulus intensity." This is, of course, a positive gain in a difficult experimental field.

The same author (39) has performed a service in reviewing the history of the past century in olfactometry. She has shown many of the failures and inadequacies of apparatus and method and has set forth clearly the desirable specifications of a good olfactometer. Foster & Dallenbach (40) have also described an "olfactorium." This is characterized by them as "the first instrument equipped to provide a completely controlled odor environment." Likening the performance of odor experiments in an ordinary room to the taking of audiometric readings on a busy street corner, they describe a glass double chamber of 250 cubic feet capacity which is supplied with controls for frequently neglected variables, such as residual odors of the subject, certain conditions of stimulation, and several specific atmospheric complications.

There has appeared, within the year, a printing of Adrian's Pedler Lecture at the Royal Institution (41). Labelled simply, "The Sense of Smell," it presents an excellent overview of our present knowledge concerning the operation of the olfactory organs. The essay is written for a public audience, in nontechnical language insofar as possible, and provides a perspective that could come only from his rich experience in neurophysiology. Reviewing the behavior of the olfactory nerve in response to mechanical and odorous stimuli and considering the anatomical and physical possibilities of stimulation, Adrian concludes that olfaction is a "patterned" sense of restricted repertoire. Different loci of impingement of odorous particles on receptor cells, varying rates of nervous discharge, variable latencies—all these provide pattern differentiation. Perhaps "specific" receptors are not present in great abundance and perhaps this is why we really have not isolated any "primaries" as yet.

The effect of sugar (42) and of amphetamine (43) upon olfactory acuity and appetite and the influence of odors on appetite (44) have been the subjects of recent communications. The first, taking as starting points the common hospital experience that intravenous glucose injection does not satisfy appetite and that direct, fistular feeding of the stomach does not

produce a feeling of satiety, comes to the conclusion that sugar is capable of changing appetite to satiety (and, incidentally, reducing olfactory acuity) only when it is both tasted and ingested. Suitable controls eliminated other possibilities. A dual gastrointestinal response is necessary to the effect. The second paper reports a positive effect of amphetamine (benzedrine) sulfate in decreasing olfactory acuity, as measured by the Elsberg technique (59), in reducing the intensity of appetite feelings, in decreasing freely selected caloric intake, and in producing a sense of satiety.

The effectiveness of benzedrine in subduing the sensation of appetite may be measurable by measuring its influence upon olfactory acuity. The functional state of the olfactory system may be important in the regulation of the sensation complex of appetite and satiety and the anorexia produced by amphetamine sulfate may be attributable to a depressant effect of this drug upon acuity of that special sense.

The third paper investigates the effects on gastric tone and motility, studied by the balloon method, of exposing various strong odors to dogs and men. Three of 12 dogs showed significant motility changes after exposures of burned hair, isovaleric acid, or springflower perfume. The inhibitory effects lasted up to 15 min. in some animals. However, 12 fasting men remained unaffected by inhalation of odors of burned hair or paper, asafetida, tincture of valerian, putrefied shrimp bran, and feces. The odors effectively suppressed any immediate desire for food but did not alter stomach tone in any significant manner.

A study of "smell blindness" and its probable inheritance has been reported by Patterson & Lauder (45). The initial subject population consisted of 4,030 high school and college students. From these, 17 cases who had originally responded negatively to a relatively weak solution of butyl mercaptan and who remained anosmic to a solution of 10 times the strength were investigated with respect to their olfactory sensitivity to other odors (vanilla, cloves, eucalyptus, oil of orange, tar, and hydrogen sulfide). Olfactory sensitivities in the immediate families were also tested. Four types of deficiency were found: (*a*) seven cases were obviously traceable to accident or disease; (*b*) four cases showed inheritance of smell "blindness" as a recessive character; (*c*) four cases evidently inherited their anosmia as a dominant character; (*d*) two cases had to be classed as "partial" on the basis of further testing, since they could get a faint odor from a strong mercaptan solution. One also had trouble with tar.

Deterioration of olfactory sensitivity as an occupational hazard is reported by Meurman (46, 47). Lime and cement dust definitely impair the sense of smell. The effect is at its practical maximum in workers of five years' length of service; after that there is no appreciable further diminution in acuity.

An especially valuable systematic review of olfaction, gustation, and "common chemical sensibility" in insects is provided by Dethier & Chad-

wick (48). Quite apart from its orderly treatment of insect behavior in response to chemical stimuli the review is useful for its comparative data on human sensitivity and for other relevant vertebrate data. There are, for example, tabular comparisons of olfactory thresholds of certain insects and man, recognition thresholds in man being compared with acceptance or rejection thresholds in insects. A similar table has been compiled for taste thresholds. Another valuable contribution of the review is its systematic identification and treatment of the experimental variables entering to complicate chemical sensitivity measurements on insects. What is known of both major and minor variables is set down and carefully documented. A bibliography of 154 titles, ranging from the classical to the contemporary, is included.

Another comparative study of "contact chemoreception" is reported by Frings (49). Similarities in differential sensitivity of caterpillars (*Eacles imperialis*), rabbit, and man are demonstrated. The relative stimulating effectiveness of various salts is determined and the conclusion is formulated that

> the cation and anion series found here are similar to those found previously for gustation in other insects and for cell penetration or surface effects, such as adsorption on colloidal particles. This suggests that stimulation of contact chemoreceptors is the result of penetration or surface activity.

*Gustation.*—An experiment in the field of taste closely paralleling the Harper & Stevens work on kinesthetic sensations (8) is that of Lewis (50). From a series of comparison solutions subjects selected the one which most nearly tasted "half as strong" as a standard solution. The standards were sodium chloride, sucrose, quinine sulfate, and tartaric acid, each being used at several concentrations and the "halving" procedure repeated. Scales were thus derived for each kind of solution. Sensation intensity, in arbitrary units, was related to physical concentration, in gm. of solute per 100 cc. of water. In general, it was found that, at the lower concentrations, the solution chosen as half strength was greater than half concentration while at the higher values the comparison stimulus selected was somewhat less than half the physical strength. Quinine sulfate was an exception, the half value being fairly uniformly selected as half. By comparing j.n.d. scales of taste with those derived by the above procedure it was found that discriminable increments in gustatory intensity increase in subjective size with an increase in stimulus concentration. This, of course, supports Stevens' requirement for the "excitation added to excitation" situation (3).

Lewis (50) pointed out that cross qualitative intensitive matches between sucrose and sodium chloride solutions seemed to be possible and further indicated that, if such matches could be obtained between solutions representing all four classical gustatory qualities, it should be possible to develop a general psychological scale of taste intensity. Beebe-Center & Waddell (51), following this suggestion, have shown that such

judgments are indeed possible. Two subjects experienced little difficulty in judging the relative strengths of solutions of quinine sulfate, tartaric acid, and sodium chloride when compared with standard sucrose solutions. Matches were also realized at several scale values, thus seeming to warrant the construction of a general scale of taste strength. To provide a unit for such a scale the taste intensity produced by a 1 per cent aqueous solution of sucrose was chosen and dubbed a "gust."

The relative sweetness of sucrose and dextrose is the theme of an experiment by Lichtenstein (52), who used a modification of the Method of Limits to compare these two sugars. The relative sweetness of dextrose proves to be a function of the concentration of the standard sucrose solution. Qualitative changes in dextrose occur as concentration is increased. At high values a bitterness complicates the sweetness; this does not happen with sucrose. Dextrose in large concentrations "digs in" and may induce burning sensations in the throat. The wisdom of retaining sucrose as a standard sweet solution in taste experiments is thus indicated.

Stimulated by Moncrieff's hypothesis (60), expressed in "The Chemical Senses" of a few years ago, that alkalinity is a function of receptors of the "common chemical sense," Kloehn & Brogden (53) have determined the limens, for eight subjects, for sodium hydroxide solution applied to the tip and mid-dorsal surfaces of the tongue. Moncrieff's hypothesis would predict no difference between the two regions yet the present authors find the tip to show reliably greater sensitivity. It is clear from the qualitative reports that alkaline solutions produce complex sensory effects and the authors believe that receptors for pain or "the common chemical sense," or both, are stimulated in addition to gustatory receptors. They thus confirm von Frey and Oehrwall's views (61, 62) that alkaline taste is a complex and not a primary taste quality, though they do not find for von Frey's "odor" nor Oehrwall's "touch" as the supplement. Pain is the more probable complicating agent.

Utilizing the phenomenon that rats normally show a preference for slightly salty water, when given a choice of that or pure water, Pfaffmann & Bare (54) have performed systematic surgical operations on the lingual and glossopharyngeal nerves in an attempt to ascertain the essential sensory control of the preference behavior. The results (reported only in abstract as yet and before the histology was completed) indicate that "some sensory mechanism other than lingual taste is essential in initiating this behavior." The preference phenomenon would seem not to be exclusively dependent on nerves subserving lingual sensitivity.

"Taste blind" identical twins have been reported by Segall (55). Both failed to appreciate PTC (phenylthiocarbamide), though they were sensitive to brucine. The cases are more interesting for the probability that their ageusia is associated with a history of diabetes than for any other possible connection, although enough abnormalities are cited (peripheral vascular disease of upper and lower extremities, bizarre neurological lesions, mental deficiency, anatomical malformations) to lead to little sur-

prise that sensory anomalies also are present. The association between PTC ageusia and diabetes has been noted before and whether the connection is an intimate one is not known. These cases, with their manifold complications, can do little to clarify the picture.

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# INDIVIDUAL DIFFERENCES[1]

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The scope of the present chapter has been defined in part by the interests of the author, in part by limitations of space, and in part by the overlapping jurisdictions of other chapters in the book. The author has tended to emphasize intellectual and cognitive function at the expense of other aspects of the individual and to give disproportionate space to studies of the organization of attributes. The more applied aspects of individual differences are slighted, except as they appear in other chapters dealing with clinical, vocational, or educational psychology.

Recent discussions of the psychology of individual differences are found in texts by Tyler (1) and by Anastasi & Foley (2). Reviews of research on intellectual development during the period preceding that covered here may be found in the triad of articles by Goodenough & Harris (3), by Worbois (4), and by Lorge (5).

## Methods and Trends in the Measurement of Attributes

Clearly, trends are not the product of any one year. The trends which hold the center of the stage at any one time had their origins years and decades ago. Thus, the aptitude batteries which have taken a prominent place on the aptitude measurement stage since the war had their roots in the work of Thurstone and of the U. S. Employment Service before World War II and in the classification program in the military establishment during the war. These batteries have multiplied since the war. The Thurstones (6) have continued to extend the age range of their Tests of Primary Mental Abilities. The Psychological Corporation has released the Differential Aptitude Tests by Bennett *et al.* (7). These tests, which were designed primarily for guidance work at the high school level, represent a compromise between the primary abilities of factor analysis and the practical categories of education and job requirements. An ambitious attempt to cover a wide range of factors, relying heavily upon wartime studies of ability factors, and to provide this coverage with pure univocal tests has been initiated by Guilford & Zimmerman (8). The Guilford-Zimmerman Aptitude Survey is planned eventually to consist of 20 tests, built on the basis of factor analysis with the primary purpose of getting pure measures. Seven tests were complete at the time of reporting. The U. S. Employment Service General Aptitude Test Battery is the end product of many years of work and of much tryout with persons working in various sorts of jobs. Dvorak (9) provides a recent report of the work

[1] This review covers the period from approximately January, 1948 to June, 1949.

on and with it. The aptitude batteries should provide the comparability of norms from test to test, the sharp differentiation of functions measured in different tests, and the broad coverage of areas of ability which are needed for an effective program either of guidance or of classification of personnel. Their practical usefulness hinges now on co-ordinated and extensive programs of test validation on a scale which may well give the private publisher pause.

A quite different fruition of wartime ability testing is found in a group of studies of abbreviated intelligence tests. In contrast with the aptitude battery, which undertakes to extend the traditional intelligence test and fractionate it into much more specific abilities, the brief screening test of intelligence undertakes to reduce the standard test to a fraction of its previous length with a minimum loss in reliability and scope. The development of these tests was accelerated by the wartime need for tests which could be individually administered to persons for whom a group test was inappropriate, but which took much less of the trained tester's time than did the usual Binet or Wechsler. An extended series of investigations has been carried out by Hunt *et al.* (10, 11), exploring previously suggested screening tests and subtests of standard tests in various combinations and relating them to the Navy General Classification Test. The test which appears to be favored in their investigations is a combination of the Comprehension and Similarities subtests of the Wechsler-Bellevue with a 15-word vocabulary test picked from the 45-word test of the Revised Stanford Binet. For a cross section of Navy recruits, this test is reported to correlate 0.75 or better with the General Classification Test. The subtest scores are reported also to have some value in clinical diagnosis. Critical scores on vocabulary test, in relation to the other two, identified 60 per cent of a group of schizophrenics at the cost of 20 per cent of a control group of normals.

Altus (12) reports results for a 13-item information test, the items for which were selected from a longer test on the basis of item analysis, as used with various Army groups. For inductees, this correlated 0.73 with AGCT and 0.64 with claimed years of schooling. In a college group, it correlated 0.64 with the Schrammel-Brannan revision of Army Alpha. Cofer (13) reports correlations with each other and with the Otis Group Test of about 0.85 for both the Kent and the Buck screening tests mental ability as applied to adolescent first offenders.

The correlations indicated in all these studies are sufficiently high to support the value of a short test made up of well-chosen items, when it is used for rough screening purposes. It should be recognized, however, that most of these studies have been carried out with quite heterogeneous groups, varying widely in education and ability. Also, most of the abbreviated tests which have been developed are individual tests, requiring the services of a trained examiner for each subject examined. They do not represent any particular economy as compared with the standard group intelligence test. The function of the abbreviated test lies primarily in

providing a rough screen for personnel who, because of illiteracy, emotional disturbance, or other reasons, cannot adequately be tested with the usual group techniques.

An interesting use of picture materials to explore the full range of verbal ability has been reported by Ammons & Huth (14). Their picture vocabulary test consists of sets of four pictures. A word is presented, and the picture must be selected to which this word is relevant. The authors found it possible to extend the test to high levels of abstractness and rarity.

An effort to measure altruism in children reported by Turner (15) extends the general approach of the Vineland Social Maturity Scale to a new area. Here, again, information is gathered by an interview not with the subject himself, but with a parent, teacher, or other person who knows the child well. A situation is described, the informant is asked to think of a recent occasion on which the subject responded to that situation, and to describe his behavior. For each of the 30 questions in the scale, the examiner rates the described behavior as unselfish, questionable, or selfish. The scale is reported to have shown a split-half reliability of 0.83 and to have yielded relatively high correlations with independent ratings of ethical characteristics.

Towards the end of World War II, the Army drastically revised its procedures for personnel rating. Central to the new procedure was a forced-choice method, in which the rater was required to choose from sets of four attributes the one most characteristic and the one least characteristic of the person being rated. Each set of our options really consists of two pairs, one a pair of favorable statements and one a pair of unfavorable statements. The members of each pair are so chosen that they are equal in general popularity but differentiated in diagnostic significance for a particular criterion—such as competence as an Army officer, as judged by a group of fellows. Sisson (16) describes briefly the development of this procedure, and the evidence for its superiority over the previous rating system. The chief values of the technique appear to lie in its ability to reduce conscious biasing of the rating by personal liking of the ratee and to minimize individual differences in interpretation of qualitative adjectives and in meaning of different scale points. The technique is being tried in various industrial situations. It is to be hoped that appraisals will be made public of its effectiveness in various situations calling for evaluation of personnel.

In the field of attitude measurement another interesting type of forced-choice test is described by Hammond (17). This uses an "information test," in which two types of attitude indicators are mixed in with a certain number of legitimate factual items. One type of attitude indicator is a factual item for which both of the response options offered are false, one falling on each side of the true answer. Thus, if the correct answer were $50, a question might be phrased: "The average weekly wage of workers in the United States in 1948 was (*a*) $40 (*b*) $60." The direction

of the error-choice is considered symptomatic of prolabor (*a*) or antilabor (*b*) bias. A second type of attitude indicator is the question with a somewhat factual flavor, for which no answer can be demonstrated to be correct, i.e., "The number of Communists in labor unions is (*a*) increasing (*b*) decreasing." Evidence of the validity of a test on labor and of one on Russia is found in the ability of each to differentiate between union and business groups. However, the method is reported to have rather low reliability for a relatively homogeneous college group. The "information set" which this procedure undertakes to induce, rather than an "attitude set," promises real value in studying attitude areas in which the individual may consciously or unconsciously be unwilling to reveal himself.

## The Nature and Organization of Traits

The dominant technique for studying the organization of the traits of individuals continues to be that of factor analysis. The techniques of factor analysis have been widely applied, and some of the applications will be considered presently. The past year has not seen major developments in basic factor analysis technique, publications on method being devoted more to clarification, to interpretation, and to interrelations of different methods.

Thurstone's current interpretation of "simple structure" calls for rotation of the original centroid factor axes into positions in which they may be oblique to one another, i.e., no longer independent factors. Several articles have been devoted to the clarification of these oblique factors, and particularly to specifying their relationship to the results obtained by Holzinger's procedures of analysis. Publications by Holzinger (18) and by Harris & Knoell (19) may be mentioned. Holzinger (20) has also presented a critique of the second-order factors which arise when the primary factors are themselves correlated. In a synthetic example, he gives evidence that second and higher order factors in oblique solution may themselves correspond to general or group factors in the orthogonal primary factor domain.

Harris (21) proposes and illustrates a procedure for getting an oblique simple-structure factor solution by graphic methods directly from the initial centroid analysis. The technique uses Thurstone's method of extended vectors and estimates by eye the intersection of the factor planes with the plane in which the extended vectors are plotted. Adjustments are made until the projections in the several dimensions are reconciled. The method appears to provide a very economical and reasonably objective way of carrying out the rotation for as many as four factors. Whether it would work out satisfactorily with a larger number is perhaps a question.

Several studies have been directed primarily toward determining the consistency of factor structure in different populations or from different sources. Dudek (22, 23), Fiske (24), and Michael (25) agree in finding many of the same factors when data from different groups and different

sources are analysed. However, in each case there appear to be some discrepancies in the factors found, and greater discrepancies in the factor loadings. Perhaps most striking are the shifts in factor loadings for a criterion measure (of pilot success), where that was included in the set of variables analysed by Dudek and by Michael. Differences in the populations studied appeared to influence the practical significance of the factor scores much more than they did the structure of the abilities measured. Harris (26) also studied the stability of factor patterns from group to group determining his rotation to simple structure upon one group and then "cross-validating" this rotation by applying it without change to other groups. Working with data from language skill tests applied to elementary school groups of Indians, he found a core of community in the factor patterns, but also some sharp divergences. The "cross-validation" technique is interesting, but somewhat unconvincing as applied to Harris' data because of the small size of his groups (N from 42 to 55). Additional data on consistency of factor patterns will be discussed presently in connection with Cattell's studies of personality factors.

Guilford (27) has presented the case for factor analysis in a test-development program in which the tests are to be used for classification or guidance, and in which the test battery must provide estimates of the relative likelihood of success in a number of different job categories. In this situation, in which it is important to have the battery cover as wide a range of traits as possible and to measure each trait in as pure form as possible, simple, uncorrelated, factorially pure tests come into their own. Factor analysis serves as a guide in identifying such tests. Guilford describes various other values of factor analysis in this situation—in analysing the nature of the criterion measure, in permitting a type of objective job analysis through factors appearing in job proficiency measures, in making possible estimates from factor loadings of the intercorrelations of tests and of criteria. In all of these analyses it must be recognized, of course, that it is the common factor variance which is accounted for in factor analysis, and that insofar as the various tests have significant or valid variance in specific factors, they cannot adequately be characterized by the primary traits of a factor pattern.

One of the troublesome problems in applying factorial methods to practical concerns is that of obtaining for each individual uncontaminated estimates of his scores on the several factors. Factor scores which are obtained are usually correlated more highly than the factors themselves, due to common tests or overlap in the factorial composition of the tests used in estimating different factors. Guilford & Michael (28) propose to attack this problem through the use of a suppression test. They report that it is usually possible to construct tests which are pure (univocal) except for the presence of one or possibly two contaminating factors. The usual contaminators are verbal, number, and perceptual-speed factors, for each of which quite pure tests can readily be constructed. The pure test of one of these factors may be used as a suppression test to eliminate this factor

from an otherwise pure measure of some other factor. Guilford & Michael give formulas for determining the relative weight of the suppression test in order to achieve various objectives in the factor composition of the combined score. The procedures would appear to be simpler than using a complete regression equation to estimate factor scores, and potentially more effective than estimating factor scores from subsets of tests.

In a critical discussion of the nature of psychological "traits," Anastasi (29) points out that trait research has suffered in the past from a "predominantly factuo-descriptive approach." As part of the search for "the underlying behavior principles which unify the superficial divergencies of the descriptive approach," she calls for studies involving "comparison of factor patterns among subjects differing in age, education, occupation, sex, cultural grouping, and species." Some such studies have been referred to above. She also speaks for a more direct approach in "the experimental manipulation of behavior organization through the interpolation of relevant controlled activities." No studies involving just the type of experimental manipulation which Anastasi had in mind appear in the current literature, but there are several attempts to relate the type of factor revealed by factor analysis to genetic, physiological, and neurological factors in the individual.

In the field of animal behavior, we have Searle's study (30) of the organization of hereditary maze-brightness and maze-dullness. Taking advantage of the distinct strains of maze-bright and maze-dull rats which were initially developed by Tryon a decade or more ago, Searle obtained a series of 30 measurements for 10 rats of each strain. Scores for the different measures were transformed into roughly comparable units by using a small control group of "average" rats as a norming group and expressing scores on all tests as normalized standard scores. The consistency of pattern within the bright group and within the dull group was shown by the mean of the correlations of each member of the group with each other member. Within groups, these correlations were 0.59 and 0.53 respectively, while the mean of the correlations of a rat in one group with one in another was $-0.18$. In other words, both the bright rats and the dull rats show a consistent profile or pattern of test performances, but there is no correspondence of the one pattern with the other. Searle then goes on to compare the median performance of maze-bright and maze-dull rats. It is abundantly clear that error score on an alley maze does not correspond to "general intelligence" in the rat, for the dull group do as well or better on a number of types of learning tasks. Searle characterizes the maze-bright rats as "characteristically food-driven, economical of distance, low in motivation to escape from water, and timid in response to open spaces," while dulls are characterized as "relatively disinterested in food, average or better in water motivation, and timid of mechanical apparatus features." The genetic differentiation which Tryon achieved appears to be on a complex motivational and emotional base rather than one of basic modifiability of response patterns.

Another attempt to integrate factor analysis with more direct physiological and neurological approaches has been made by Halstead (31). Halstead administered a battery of 26 tests, chosen for their potential value in illuminating the effects of cortical destruction, to various groups of clinical and brain-injured cases. Using a group of 50 subjects, described as "healthy adult males . . . medically recovered from a recent concussive type of head injury," Halstead computed intercorrelations among 13 of the test variables and had carried out (by Thurstone) a factor analysis eliciting four factors. On the basis of these limited data, an elaborate and rather dogmatic picture of intellectual functioning is drawn. Some effort is then made to study the effect upon these factors of such influences as anoxia and cerebral injury. However, most of the analysis of injury effects is related not to loss in the separately-identified factors but to an undifferentiated "impairment index." Research of this basic pattern, in which an attempt is made to study the effect upon factor scores developed by statistical factorial techniques of describable physiological and neurological changes, is much needed. Halstead's study falls far short of being satisfactory, however. Neither the sample of tests nor the sample of subjects was adequate to provide a clear and definitive factor analysis; there was no clear, systematic attempt to relate each of the factor scores to the physiological and neurological modifications which were studied.

As a third attempt to relate factor analysis results to data from a variety of other sources, we may mention the work of Eysenck (32). Working with groups of hospitalized neurotics in the British Army, he carried out a factor analysis of a group of items on the patient's record sheet. Four factors were extracted, but only the first two were interpreted. The first of these was considered to be a factor of general neuroticism, while the second was a bipolar factor labelled hysteric-dysthymic, and considered to correspond to the dimension extravert-introvert. The author then explored a great variety of measures, to see whether the personality dimensions which he had identified were supported by other behavioral, physiological, and morphological differentia. A number of measures were found which yielded significant differences, some for the normal-neurotic and some for the hysteric-dysthymic dimension.

There have been, during the period covered by this article, a number of studies of cognitive functions by factor analysis methods, only a few of which can be mentioned here. Burt (33) has initiated a summary and interpretation of the results of factor analysis of the cognitive functions, which, when completed, should provide a useful integration of this work.

A study by Rimoldi (34) is concerned primarily with fitting Raven's Progressive Matrices test into the framework of factor analysis. Five sections of this test were given along with a number of other performance-type tests, e.g., form boards, man-drawing, cancellation, mazes, etc. The emerging factor pattern was not too readily interpreted, and the absence of measures of a number of the better-established factors precluded relating these results to the familiar reference axes. The factors which were

brought out in the analysis were considered by the author to be primarily perceptual in character, and may serve to re-emphasize the role of perceptual factors in intellectual functioning.

The general area of "fluency" has been explored in studies by Fruchter (35) and Taylor (36). Though differing in detail, these studies agree that "fluency" is a composite of at least two factors. Taylor identifies factors of word fluency (*W*) and of ideational fluency (*F*). Within the general verbal area, Fruchter speaks of two, possibly three factors. "Fluency in dealing with single words" and "fluency of association for common words where there is some restriction placed upon the response" are perhaps subdivisions of *W*, while "rate of production of syntactically coherent discourse" seems closely related to *F*.

The existence of a common speed factor in mental test materials is explored in an interesting study by Tate (37). In this investigation, response times were taken separately for each individual test item. Four different intellectual tasks and three difficulty levels were used. The resulting scores were studied by analysis of variance, and significant variance was found associated with individual, type of test task, difficulty level, and correctness of response. Intraclass correlations allocating variance to individual differences ranged from 0.60 to 0.80. However, the analysis does not make clear to what extent the speed factor was general, the individual maintaining the same status in his group in all types of tests. Correlations between scores for speed and estimates of level of performance were low.

The period surveyed has been particularly prolific in factorial studies in the field of personality. A large part of this material consists of a series of reports by Cattell. Cattell's program of research on personality factors, which earlier gave rise to his book, *The Description and Measurement of Personality*, has continued in a related series of factor analysis studies in this area. He has undertaken to bring together studies using ratings by associates, self-evaluation by personality questionnaire, and performance tests. The objective has been to bring together these domains of investigation in order to see to what extent the same factors appear in all, and to identify through the more directly interpretable ratings the functions assessed by objective performance measures.

Two reports (38, 39) present the results of studies using rating procedures, where each subject is rated by a group of about 15 associates living in close proximity with him or her. One study deals with a group of college men and the other a group of college women. Thurstone's methods of analysis are used, and a laborious oblique rotation to simple structure is reported. Twelve and 13 factors were extracted for the 36 variables in these two studies, and 11 were interpreted in each case. Nine or 10 of the interpreted factors were considered by the author to coincide with those of the other studies of the series. However, the identity of the factors in the different studies is far from complete. The author is satisfied to consider a factor appearing in two studies as the same factor if three out of

six of the variables with highest loadings are common to the two studies. The reader may be somewhat hesitant to accept Cattell's characterization of the different factor dimensions, may be somewhat disturbed at the complexity of the correlated and descriptively quite complex factors, and may be rather less prepared than the author to accept the factors in different studies as identical. In this connection, the reanalysis of Cattell's data by Banks (40) may be of interest. In any case, the work must be acknowledged as an impressive attempt to study the whole personality sphere.

A third report (41) deals with the investigation of objective tests of personality attributes. This also was an ambitious enterprise, in which 100 performance measures were assembled and administered to the same subjects who served in the previous studies. With this number of measures the tests were necessarily brief, and many of them were of doubtful reliability. Because of the labor involved, only 48 of the variables (those which seemed most promising) were analysed. The table of correlations which is reported shows the bulk of the correlations to be very low, and many of the tests have very low communalities. (The median communality is about 0.17.) Twelve factors were extracted and rotated, and an attempt was made to characterize these factors. However, many factors were represented by only small loadings, so that the identification of them with factors from previous studies or the verbal characterization of them seems tenuous at best. About a third of the tests had no appreciable loadings on any of the factors.

This last study was admittedly an exploratory one. It does perhaps raise the question of how low reliabilities and communalities may be and still permit a factor analysis which can yield clear and interpretable results. The writer has the feeling that with communalities as low as those found in this study, the small amount of variance which is represented by common factors is almost sure to be obscure and unclear. In these studies, Cattell has ambitiously undertaken to plot out the whole sphere of personality variables, and the representation of any sector of the sphere has necessarily been limited. It is to be hoped that more intensive exploration of more limited segments will serve to refine and, if necessary, to revise the picture which has been sketched for us.

Cattell and associates (42) have extended the realm of factor analysis from the study of a population of individuals to a population of measurements of the same individual. Data were gathered for a particular individual at 55 different times spread over a nine-week period. About 15 objective measures were obtained of such things as PGR, body-sway, reaction time, etc. Ratings and self-ratings were also obtained. A two-stage analysis was carried out, first of the objective test scores and then of factor scores from this analysis together with the rating variables. The results were felt by the authors to fit rather well into the structure of personality factors previously built up in studies of groups of individuals, though the matching is not exact.

One further study from Cattell's laboratory may be mentioned at this time, though it relates only in a very general way to the studies which have just been discussed. This is an extension by Cattell & Wispe (43) of the methods of factor analysis to the study of group performances, following up an examination by Cattell (44) of the possibility of such an application and the conditions to be met by a study. An ingenious and diverting series of tasks was planned for a series of 3-hr. group sessions in which college women worked in groups of six competing for a prize. Forty-one scores were obtained for each of 21 groups. For a sample of these tasks the reliabilities were computed, and the median of the values reported was about 0.30. Intercorrelations were computed, a factor analysis carried out, and an interpretation of the factors attempted in terms of the previous analysis of individual personality traits.

The reviewer found the results from this study even more than usually confused and difficult to interpret. This is perhaps not surprising in view of the small N and low reliability of the separate measures. A study such as this raises in the reader's mind the question of how crude and meager data may be, and yet merit analysis and reporting. Recognizing that the study represents an exploration of a new area, and recognizing also the extraordinary amount of time and labor involved per case, we may still question whether our general scientific understanding is forwarded by carrying out and reporting a factor analysis of 41 variables, many quite unreliably measured, for a total of only 21 cases. The sampling errors and errors of measurement are so large, the degrees of freedom so limited, that the results can hardly help being confused. The results from analysing such limited data are likely to include so much error with the truth that they seem likely to retard rather than advance our understanding. Though this was the smallest N encountered, the writer was distressed to note the number of factorial studies still appearing which erect an imposing edifice of analysis upon a meager foundation of empirical data.

Several other studies in the general personality-temperament area will be commented on only briefly. Moore, Stafford & Hsu (45) carried out an obverse analysis of personality, that is, an analysis in which traits and persons exchange places and each correlation coefficient refers to a population of traits and a pair of persons. The finding which emerges from their work is that if clear personality-type factors are to emerge, which correspond to clusters of very similar persons, those trait items which are common to almost all persons in the group must be eliminated from the population of traits. These common attributes appear to blur the emergence of distinctive personality types.

Wenger (46) reports factor analyses of a number of physiological measures relating to autonomic balance, as part of a general report of the use of these tests with Army Air Force personnel. Most of the correlations among variables were extremely low, but it was possible to extract five common factors. Three of these were interpreted as autonomic, blood-

XUM

sugar, and thyroid factors respectively. The autonomic factor had fairly substantial relationships with certain personality inventory scores.

Burt (47) reports a factorial analysis of temperamental traits for a group of normal children and a group of psychoneurotic patients. He finds (*a*) a general factor of emotionality, (*b*) a bipolar factor distinguishing sthenic from asthenic emotions, (*c*) in the normals, a bipolar factor distinguishing euphoric from dysphoric emotions, and (*c'*) in the psychoneurotics, a factor suggesting four overlapping groups roughly identifiable with anxiety states, obsessional states, hysteria, and neurasthenia.

Burt (48) also brings together the results of a number of studies of factors in bodily measurements. The data, he believes, are particularly amenable to analysis in terms of his methods, which yield a general factor and group factors. Typically, studies of bodily measurements yield a substantial general factor of size, a bipolar factor distinguishing vertical from lateral size and corresponding to the concept of body type, and more specialized factors of trunk size, limb size, head size, etc.

Eysenck (49) reports an analysis of attitude statements, yielding bipolar factors identified as radicalism versus conservatism and tender-mindedness versus tough-mindedness. The identification of the first of these was validated by correlating the factor loadings of individual statements with the size difference between their frequency of endorsement by members of the Conservative and of the Labor party. The correlation was 0.98.

Hsu (50) carried out an exploratory study in which responses by different individuals to one of the Rorschach cards were submitted to factor analysis. Due to the fact that the categories of response which were analysed were not experimentally independent, certain ones being included as component parts of others, the results were not too meaningful. The interplay of factorial and projective methods represents a line of development which seems worth noting.

### Stability and Modifiability of Attributes

The present period sees the maturing of results from longitudinal studies which were started in the 20's and 30's. In one study by Neilson (51), the babies extensively studied by Shirley were followed up 15 years later. Personality sketches of 15 of the children written on the basis of observations made during the first two years of life were compared with sketches prepared independently on the basis of tests and ratings given at the age of 17 years. Matching of the infant versus the adolescent sketches was reported significantly above chance, so that recognizable personality patterns persisted over this period of time. Some individuals were more readily identifiable than others, presumably due to the greater uniqueness of their personality pattern.

Reports of the results from a series of individual intelligence tests administered to the same children at intervals, starting in infancy and con-

tinuing to the age of about 18 years, have been presented by Bayley (52) and by Honzik, Macfarlane & Allen (53). These results agree in indicating that infant tests have essentially no predictive value for school-age intelligence, that tests close together in time are most closely related, and that the function measured by intelligence tests stabilizes in the early school years, so that tests at age seven or eight give almost as good predictions of the terminal intelligence measure as tests at any later age. This finding is in accord with Thorndike's report (54) that score on the College Entrance Examination Board Scholastic Aptitude Test is predicted as well by a test at the beginning of high school or even in the later years of elementary school as by a later test.

Embree (55) has related childhood IQ, as represented by the median of five group test IQ's expressed in terms equivalent to 1916 Stanford Binet IQ's, to subsequent educational history. Following up the college careers of 1,200 University of Minnesota High School graduates, he found the mean childhood IQ of college entrants to be 118, of recipients of bachelors degrees to be 123, of advanced degree holders to be 126, of honor graduates to be 133, and of Phi Beta Kappas to be 137. IQ's of persons who received degrees ranged from 95 to 180. Equating his test scores with American Council Psychological Examination scores Embree estimated a number of parameters of the college population to be as follows: national freshman average IQ, 113; average IQ in highest and lowest colleges reporting A.C.E. results, 133 and 100 respectively; average for teachers' colleges, 109; 90th and 10th percentiles of the national college group, 133 and 100 respectively. These equivalences should help to give some additional meaning to childhood IQ's especially for the upper 25 per cent of the IQ distribution.

The problem of the effectiveness of social and educational influences in modifying psychometric and functioning intelligence continues to attract attention. The long series of studies initiated at the University of Iowa is represented in this period by a study by Skeels & Harms (56), in which the most recent data on IQ's of foster children from the Iowa Soldier's Orphan's Home are reported. The foster children of purportedly feeble-minded mothers continue to score somewhat above 100 IQ on the average. Only slight relationship is found either with true parent IQ or foster parent education. In general, the results continue to follow the pattern of the earlier Iowa studies.

Perhaps the most interesting large-scale opportunity for studying the effects upon measured intelligence of the general increase both in formal education and in mass media of communication is found in intelligence test data for this country in the two World Wars. Tuddenham (57) makes available the results from one study on the cross-standardization of Army Alpha and the A.G.C.T. Data were obtained for 768 men who were selected to constitute a representative sample of the total World War II draft population. These men were given the Wells revision of Army Alpha. Though this is not the original Army Alpha, the author indicates that it

yields approximately the same distribution of scores. Accepting these scores at their face value, we find that the median draftee of World War II falls at about the 82nd percentile of the World War I distribution. This would represent a shift of very nearly one standard deviation of the World War I scores, or the equivalent of perhaps two years in Binet mental age. Clearly, the increase in education (10th grade on the average, as compared to 8th), the sound film, the radio, and the other changes of the 25 year period have made a substantial difference in the performance of our population upon the usual type of group intelligence test.

The most recent startling claims for the effectiveness of special educational procedures are those of Schmidt (58), who purportedly brought about very striking changes in the psychometric intelligence and social effectiveness of children who initially appeared to be feeble-minded. The distinctive feature of her treatment is not entirely clear, and critics have contended that the program which she described is not essentially different from the type of individualized, child-centered program which enlightened schools have been providing for children in special classes for years. Hill (59), for example, fails to find any evidence for average gain for children in special classes in the Des Moines schools, whose program has "accepted 'experiential learning' as good instruction and . . . developed around the elimination of frustrating influences and the promotion of a more adequate personal and social development of the children." Critical reviews of Schmidt's article have been published by Goodenough (60) and Kirk (61), and a symposium of comments has been assembled by Nolan (62). These appraisals point out a number of arithmetical errors among those of the quantitative data which can be checked from the information given in the original article. A number of points of method are criticized. In the light of these considerations, Schmidt's claims must continue to be viewed with extreme skepticism until they are independently verified by competent workers.

Of chemical agents which may influence intellectual development, the two which have attracted special attention during the period surveyed are thiamine and glutamic acid. The results with thiamine are not particularly encouraging when additional amounts of this substance are added to a diet in which the supply is already approximately normal. Harrell (63) has reported a follow-up study of an orphanage group for which the initial experiment had suggestive positive results following the addition of thiamine to the diet. The original experiment had utilized an experimental and a control group. In the continuation of the experiment half of the pairs were reversed, the experimental now becoming the control case and vice versa. There was no general or clear-cut tendency for the new experimental groups now to show the greater gains in performance, and in general differences were quite small. Bernhardt *et al.* (64), working with identical twin pairs, failed to find positive results from the addition of supplementary thiamine. Neither of these experiments should be considered as relevant to the effects of severe thiamine deprivation.

Studies on the effect of supplementary glutamic acid have been reported by Bakwin (65), by Gasorek (66), and by Zimmerman, Burgemeister & Putnam (67). The groups were fairly small and the cases had not been followed up for very long periods of time. However, the results of the studies agree in showing positive effects from the experimental treatment. The chemical agent appears to be one whose action on the central nervous system and on functioning intelligence will need to be thoroughly and critically explored.

### Differences Associated with Race, Nationality, Age and Sex

The last year or two seem to provide no studies which, because of their scope, design, or special controls, add crucial evidences on psychological differences associated with such variables as race, nationality, age, and sex. There are a number of reports of work in this field, but they appear in most cases to reaffirm established results, or to be limited in scope or in method. A few of the more interesting will be mentioned.

In the field of national differences, Kaldegg (68) presents a rather objective analysis of the results obtained from a partially-structured projective method for groups of British and German boys. A single ambiguous picture was used as the stimulus for a rather extended inquiry. The responses to each question were grouped into categories, and the frequencies in the categories compared for the two national groups. Differences were quite striking in many cases. These were organized into a composite picture of the dominant personality pattern in each culture.

A somewhat similar study by Sarhan (69) compared American and Egyptian children on the basis of their response to questions concerning their three wishes, happiest day, etc. Many trends were found common to the two cultures, but certain striking differences served to indicate the impact of the culture upon the children. It appeared that failure to express interest in a particular object or experience might represent either lack of exposure or satiation, so that the children's own lack of expressed desire could not be taken at face value.

The age curve for intellectual development, particularly as it appears for Raven's Progressive Matrices Test, has been examined in a series of British studies, of which those by Slater (70) and by Raven (71) may be taken as representative. Data for the Progressive Matrices Test and for the Mill Hill Vocabulary Test were tabulated for groups of all ages up to about 60. Some groups were obtained from schools, some from the army, some from civilian employees of the telephone service, and some from an industrial organization. The controls to guarantee equivalence of sampling at different age levels are not too satisfying, so the results cannot be considered crucial. However, they agree well with earlier studies of age changes in intelligence in that they show no loss in vocabulary until after middle age, and only a slight one even then, while the Matrices Test, which involves unfamiliar adaptations to novel materials, reaches a maxi-

mum in the teens and drops steadily after the middle twenties. The smallest age drop is found for the better-educated and higher-scoring individuals.

Growth of mental test performance during adolescence has been studied by Vernon (72) and by Thorndike (73). Vernon's cross-sectional studies of tests used in Britain in the Royal Navy indicated that general intelligence showed only slight increase from 14 to 17 for those not attending school, and a drop after that for men in lower level occupations, while gains were greater and longer sustained for those in school and in higher level occupations. When there was no further education, educational attainments declined rapidly. By contrast, there continued to be a marked increase in performance on mechanical and spatial tests. The results point to the dependence of ability measures upon the use which is provided for the abilities. Thorndike's results, which indicated gains upon the A.C.E. Psychological Examination up to 20 or later for a superior group of individuals still attending school, are consistent with this interpretation.

Studies of psychological attributes of the aged have increased in numbers in recent years. The period surveyed here has brought forth a number of studies in this area. The problem of representative sampling becomes particularly acute with this group, and in most of the studies examined the samples appeared neither large nor particularly representative. A small scale study of Wechsler-Bellevue and Rorschach results was reported by Chesrow *et al.* (74), a study of vocabulary by Fox (75), one of the frequency of incidence of Jung's personality types by Gray (76), and one of attitudes and activities by Gardner (77).

Chen & Chow (78) carried out a factor analysis of a battery of cognitive tests as applied to different grade groups from fourth grade through college, and Doppelt (79) factor-analysed the Differential Aptitude Tests for groups at each age from 13 through 17. Neither of these studies supports the earlier contention of Garrett that abilities become more specialized with increasing age, and that the general factor becomes less important. In these studies, the trend is if anything the reverse, the general factor becoming more rather than less important. This problem appears to be one which still requires further clarification, if apparent contradictions in experimental results are to reconciled.

Studies of intellectual differences associated with occupation or with parental occupation continue to be made as by Stewart (80) and by Havighurst and others (81, 82). They continue to show the same substantial group differentials, corresponding to a correlation of perhaps 0.30 or 0.40 between an index of socioeconomic status and one of intelligence, and the interpretation of the results continues ambiguous, as pointed out by Davis & Havighurst (83), because of the inequalities of opportunity and culture in the different socio-economic levels. There is some evidence that the group differences are sharpest in the verbal and numerical abilities which relate most directly to formal education.

### Various Special Groups

There are available a number of studies of special groups, such as veterans, delinquents, physically and mentally handicapped, intellectually gifted, and the like. A few will be selected for mention here.

The group of veterans has been a temporary special group arousing a good deal of interest. A number of studies have investigated the success of the veteran, with his greater age, his presumably more mature interests, his background of sometimes traumatic experiences, and his years of separation from the educational scene, in college work. The studies, of which Schrader & Frederiksen's (84) may be taken as representative, agree in finding that the veteran does at least as well as the nonveteran of comparable intelligence. Another group of some interest is the group of conscientious objectors, who were compared with Army and Navy enlisted personnel by Gory & McClelland (85). They were found to be taller, heavier, older, better educated, more intelligent, and from a higher occupational level than the enlisted personnel.

Studies of delinquents continue to disagree as to the intellectual level of this group, the study by Wedeking (86), for example, emphasizing the normal intelligence of most of the group, while that by Dunsdon (87) reports a large proportion of borderline and defective intellects. In the field of delinquency, the report by Zakolski (88) presents a picture of amazing success in differentiating delinquents from non-delinquents by a self-report inventory. A 240-item questionnaire is reported to differentiate the two groups to the extent represented by a biserial correlation of 0.998! Needless to say, this result, which is reported in a somewhat confused article, and which is not supported by any independent verification, should be looked upon with extreme skepticism.

A report emerging slightly before the period covered by this review, meriting mention because of its significance, but impossible to review because of its scope and the space limitations, is Terman & Oden's latest follow-up (89) on the California group of intellectually gifted children. It is of unique importance among recent studies in this area and should certainly be read by anyone interested in the intellectually gifted.

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# PERSONALITY[1]

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Theory and research in personality fall into three main categories. The first relates to development, i.e., to the processes and conditions that create motives and traits. The second covers the dynamics of action, including reactions to frustration, conflict, and other factors that influence on-going behavior. The third is structure, the analysis of the substantive components of personality.

These categories are by no means mutually exclusive. The same event may be considered from all three standpoints. A frustration in childhood may be examined as a determiner of the strength of a developing motive, as an antecedent condition for a variety of adjustment techniques, and as a special condition for the measurement of trait factors. Principles of development and of dynamics can be stated within the same systematic framework, although the types of research that characterize the two fields are usually somewhat different. Developmental studies are longitudinal in the main, covering several years of a child's life, while studies of dynamics are more frequently limited to brief experimental situations more susceptible to rigorous control. Structural analyses are more commonly done with adults and the prevailing techniques of investigation are essentially statistical. From a systematic point of view there is nothing inherently dissimilar about the conceptualizations or research procedures of the three areas, but at the present time they are still sufficiently distinct to make their differentiation a convenience in exposition.

## Personality Development

Historically, the greater bulk of theory about the development of human motives and traits has come from psychoanalysis. While this has led to a rather one-sided interest in psychosexual development and infantile drive socialization, it has also provided a useful body of hypotheses for investigation. Freudian theory has been outside the mainstream of contemporary psychological thinking, however, and has relied on an implicit theory of learning that Freud nowhere formulated in precise terms. As Blum (1) has shown recently, this failure to make the processes and conditions of development explicit creates difficulty for the researcher who wishes to derive hypotheses sufficiently exact to permit empirical testing.

In an effort to reduce this difficulty, Misbach (2) examined Freud's writings in terms of their implications for a "central" or "peripheral" theory of learning. His conclusion that Freud was essentially a "centralist" rested on the role of the ego as an already-organized rather than a

[1] This review covers the period from April, 1948 to June, 1949.

to-be-organized structure. Misbach also cited Freud's references to associative learning but discarded them as "superficial."

Although Misbach's paper shows evidence of considerable sophistication in Freudian theory, the paper suffers the indecisiveness of most Gestalt contributions to learning theory—it stops where it should begin. The existence of "organization" prior to learning is accepted as a significant theoretical fact, since organization provides the setting for "differentiation." But the precise conditions under which differentiation occurs are not stated. Since this is the main problem with which learning theory must cope, the Gestalt approach remains uninformative. Whether Freud had a central or peripheral theory of learning seems of less significance than the discovery of the implicit principles by which he derived motivational changes from antecedent experiences.

There is some question whether this latter task can be accomplished. Freud developed his thinking on these matters before the manifold requirements of a theory of learning were understood, and it is likely that systematization of the field of personality development will stem from more modern theories than that of Freud. A rather primitive and oversimplified attempt at such a formulation was presented by Sears (3), who took as a starting point the current systematization of knowledge about secondary drive formation. With the principles of social learning to account for the processes of behavior change, he introduced hypotheses as to the nature of childhood experiences evolving from cultural demands and parent-child relationships that would provide the necessary conditions of learning for adulthood. These conditions are composed primarily of the behavior of those social persons (betas) who serve as the stimuli, manipulanda, and rewarders (or punishers) of the child's (alpha's) behavior. How successful this approach will prove will depend on the degree to which the hypothesized conditions of learning can be measured.

Of the many aspects of the psychoanalytic theory of development, none has elicited so much interest as that of the influence of early infantile experience. Psychologists faced with the uniqueness and complexity of personality even during the preschool years of life appear to have turned away, in recent years, from the magic of constitution and heredity as explanatory concepts. In their place has been put infantile experience—things that happen so early, so intangibly, and so pervasively that they seem to be possible sources for otherwise inexplicable facts. This flight into the child's mysterious past has led to considerable uncritical speculation among some pediatricians and psychiatrists, particularly in connection with early feeding experiences; and Orlansky's critical review (4) of the scientific evidence for such influences on later personality has served as a happy corrective.

Orlansky made a careful search for objective studies that evaluated the effects of breast feeding, mothering, demand vs. scheduled feeding, physical restraint, and neglect. He found the evidence extremely meagre and entirely unconvincing as support for any hypotheses that relied upon

early experience as an explanation of gross and important personality variation in later years. His coverage of the anthropological literature added data that made clear his conclusion that infants can be subjected to widely varying treatment and yet show apparently normal development. This paper deserves thoughtful reading for it provides the base-line from which research in this field must start.

Research on oral development has added a few facts of significance during the year. One of the major theoretical problems has been the origin of the oral drive. The earlier work of Levy led easily, though not necessarily, to the conclusion that the oral drive was largely a constitutional source of behavior. As such, it required gratification; and Levy had been led to the conclusion that if a child failed to secure sufficient satisfaction by nutritional sucking, he would seek it through thumb-sucking or the use of a pacifier. Data on thumb-sucking seemed to substantiate this view.

There remained a question, however, as to whether the oral drive was completely separate from the hunger drive, or whether the former was in part a secondary drive established (or strengthened) through the almost universal rewarding of the sucking act by food-getting. Two studies have added information on this matter. Davis *et al.* (5) were able to compare the oral behavior of three groups of 20 infants each who had been fed, respectively, by cup, bottle, and breast during the first nine days of life. Cup feeding provided virtually no reinforcement of the sucking response; bottle feeding probably provided little because of the type of nipples used; and breast feeding provided repeated reinforcement. The infants were tested twice daily for the strength of the sucking response and for frequency of oral movements. The breast fed group showed a definite growth in strength of the sucking response, but none of the groups presented any significant increase in oral movements. It was concluded that the reinforcement of sucking by food-getting increased the strength of response and this created the possible condition for strengthening the oral drive.

That the oral drive is not independent of the hunger drive was further demonstrated by Kunst (6). In a careful study of thumb-sucking among a large group of institutionalized children during the first year of life, she found that the frequency of sucking was greater, even at the end of the first year, when the child was hungry than immediately after meals.

Evidence was accumulated, too, that the so-called oral drive cannot be considered the sole cause of thumb-sucking or nail-biting. Kunst observed significant differences in frequency of the former depending on the presence or absence of adults in the room. Among college students, Coleman & McCalley (7) found a high proportion of nail-biting apparently due to nervousness and anxiety. This substantiates the earlier findings of Billig (33).

Some attempt has been made to trace into adulthood the consequences of early variations in oral experience. Abraham, Freud, Glover, and other psychoanalysts had observed among their patients certain con-

stellations of adult traits that appeared to result from the full satisfaction of oral strivings in early infancy or the rather severe dissatisfaction of them through early and traumatic weaning. The former seemed to create an adult character of optimism, generosity, sociability, and a willingness to accept new ideas. The latter produced pessimism, passivity, narcissism, and social aloofness. If satisfaction for the ungratified oral drive was sought at the oral-sadistic level, the adult character would also be hostile and addicted to verbal aggression.

Goldman (8) secured self-ratings on 19 trait scales from 115 young adults. The scales were largely drawn from Murray's *Explorations in Personality* (34) and provided measures of most of the traits relevant to the so-called oral character. Unfortunately Goldman used such economy of detail in reporting her statistical analyses that it is impossible to evaluate the rather complex procedure and reasoning by which she was able to separate two extreme groups corresponding to the two types of character. It is evident that the predicted scales did correlate positively and that subjects at each extreme of the distribution of "clusters" could be drawn out. Data were presented showing the average standard score of the orally satisfied and orally unsatisfied groups, 20 extreme cases in each, for each of the 19 rating scales. Comparison of these scores shows the kind of clustering required by the theory of oral character formation. It is a misfortune that these rather significant findings were not presented with sufficient clarity and explanation of the mathematical devices used that some confidence might be placed in them. The author promises a later report on her study of the early feeding experiences of the two extreme groups.

According to the theory of oral development, not only may total character structure be determined by such early experiences, but specific oral motives may be expected to persist in the adult. The same would be true of anal, phallic, and other motivational systems, or conflicts between systems, that create problems of adjustment in early childhood. In an ingenious effort to discover whether predictable consequences of early experiences could be found reflected in the fantasy activities of young adults, Blum (9) constructed a series of 10 pictures relating to the emotional and motivational experiences of a family of cartooned dogs. Blum believed that story interpretations of the dog family's behavior would be more revealing of unconscious motives than would stories about pictures involving human beings, because the anxiety aroused by dogs would be less and the dogs existed in a purely fantasy universe. The results of the study, which is not limited to oral behavior but includes behavior deriving from anal, oedipal complex, genital, sibling rivalry, and other motives, amply bear out his hopes.

The hypotheses tested were of two main kinds, sex differences and intercorrelations among motives, or what Blum called syntactical relationships within the theory. Among the significant sex differences predicted by theory and confirmed by findings on the "Blacky Test" (Blacky being the child dog with whom a subject was led to identify himself) were these:

greater expression of oral sadism and less of anal sadism in girls, greater retention of preoedipal components in girls, greater attribution of aggression to sex behavior in boys. Evidence was also obtained supporting the differential sources or agents of identification. Syntactical hypotheses included several relating to the positive intercorrelations among pregenital components of the libido, and to the roles of identification and aggression in the oedipal and sibling rivalry situations.

A somewhat less theoretically oriented approach, stemming nonetheless from psychoanalytic prepossession with early infancy, is represented by the cross-cultural comparisons of Benedict (10). Extensive areal studies of Eastern European cultures produced broad information on the practice of infant swaddling. Benedict and her anthropological associates found characteristic differences among Russian, Polish Gentile, Polish Jewish, and Rumanian mothers in their motives for swaddling—a kind of overt behavior general throughout the area. Evidence seemed to point to motives of protecting the child from his own wildness (Russian), of the development of "hardness" (Polish Gentile), of increasing feelings of warmth and affection (Polish Jews), and prevention of masturbation (Rumanian). The implication drawn from the data, which were not reported in detail, was that national character is a function not of the overt details of child-rearing but of the attitudes and motives communicated to the child by the mother, the culture transmitter, in the course of caring for him.

While these various studies of psychoanalytically oriented hypotheses have had the virtue of a more or less self-contained theoretical system, they have also been limited to a rather narrow set of problems. Continued effort by Baldwin (11) has broadened the findings of parent-child relationships from the extensive data of the Fels Research Institute. In a summary report, Baldwin has brought together certain of the more significant constellations of antecedents, in the home, of child behavior. The Fels group obtained ratings on the Champney scales of a variety of aspects of the home atmosphere of their experimental group of more than 100 children. Observations of the children have been made both by home visitors and by observers in a nursery school group. Previous publications by Baldwin and his associates have described the measurement methods in detail; the present report serves to summarize some significant relationships within a subgroup of 67 families.

The effects of variation in two main dimensions of family behavior were examined with respect to the children's nursery school behavior. Democracy-autocracy and permissiveness-restrictiveness (control) were found to vary fairly independently of one another. Children from democratic homes were more aggressive, socially reactive, and venturesome than children from autocratic homes. High permissiveness accentuated this picture. Control, on the other hand, with democracy held constant, tended to reduce curiosity and increase docility without increasing aggressiveness.

## Action Dynamics

*Displacement.*—Experimental study of psychoanalytic concepts has been difficult partly because the theory in which they are embedded lacks the precisely defined variables which are necessary to make effective tests of relational hypotheses. A notable advance in this direction has come from the work of Miller (12), whose analysis of the process of displacement was based on the principle of stimulus generalization. Since the latter process is an integral part of stimulus-response learning theory, Miller's analysis at once opens displacement to experimental investigation.

Miller had shown previously that the slope of the generalization gradient is steeper for an inhibitory than an excitatory response. In the present study he has derived the consequences of this difference and shown that the relative strengths of the two determined the degree of similarity that will evoke the excitatory response. Experimental data reported demonstrate stimulus generalization from one drive to another (thirst to hunger, hunger to pain) and from one stimulus object to another. The latter involved a fighting response in white rats, and generalization to a doll was displayed.

The several theoretical derivations in this paper suggest a series of experimental analyses that may now be made for the determination of the exact conditions under which such factors as repression or nature of the drive influence displacement.

*Projection.*—A major problem in the study of frustration is that of determining the conditions under which various types of reaction will occur. Following Freud, there has been a tendency to assume that when a need is unsatisfied, i.e., when there is some interference with the customary instrumental activities used to achieve gratification, fantasy responses that serve as substitutes will be exhibited. This principle, which seems well substantiated by casual observation, has been used as the basis for the interpretation of many so-called projective tests.

In a series of studies of the influence of needs on fantasy productions, McClelland and his collaborators (13 to 16) varied the strength of needs experimentally in order to examine systematically the kinds of changes that ensued both in story constructions to ambiguous pictures and in reported objects seen on blank lantern slide projections under positive suggestion. Two drive systems were studied, hunger and the need for achievement. The former was controlled by using groups that differed in hours of food deprivation. Achievement drive was varied among different groups by the use of different kinds of ego-involvement instructions and induced failure. All subjects were young adult males.

The fantasy responses were analyzed systematically as to whether they included reference to drive states, instrumental activities, deprivation conditions, anticipatory goal responses. The higher drive (or need) strengths produced much larger proportions of fantasies that related to the drive in question, but the various categories of response differed radically from one another in their methods of exhibiting this influence.

Increase in the strength of the hunger drive was accompanied by an increase in fantasy references to food-getting instrumental activity, but a decrease in references to food-getting goal responses. There was an increase also in food themas and in food deprivation themas, and a corresponding decrease in friendly press (invitations to dinner, etc.). In the case of need for achievement, a secondary drive for which the exact nature of the goal response is unknown, the results were similar in most respects, except that the goal responses also increased. It is perhaps worth noting that the behavior called the "goal response" for a secondary drive is ordinarily only the terminal aspect of an instrumental sequence and is often described in terms of some environmental event which the instrumental activity has been designed to create. There may be some question, therefore, whether there is any real systematic disparity in the results from the two drives. One important category that decreased under high achievement drive was that of unrelated imagery; in other words, not only did a larger proportion of stories contain reference to achievement, but those that did were more saturated with it and contained less other material.

Methodologically these studies by McClelland represent the most important advance in the field of contentual projective testing since Murray's original description (35) of the Thematic Apperception Test (TAT). Not only are they excellently designed, but they are based on a systematic behavior theory that permits the analysis of fantasy processes within a set of formulations that are also effective in the analysis of motivation and overt adaptive behavior. They eschew the static response-response correlation method of validation (test against test) in favor of analysis in terms of the dynamics that properly describe changes in behavior as consequences of changes in motivation.

A somewhat similar approach, in this instance to the study of projective doll play, was used by Yarrow (17) in an experiment in which the controlled drive was that produced by frustration. Three groups of 10 preschool-aged children each were given a half hour neutral control doll play session first, and a day or two later one group repeated under the same conditions, one was subjected to an impossible task with failure stimulation preceding doll play, and the third was subjected to frustration by over-satiation with an initially interesting motor task. Yarrow found that the frustration-induced drive (failure) increased both nonstereotyped and tangential aggression. Other measured aspects of the doll play were not influenced by the frustrations. Yarrow's conclusions are similar to McClelland's in one respect; he emphasizes the importance of knowing a subject's immediately preceding experiences before any attempt is made to interpret behavior in a projective test situation.

*Repression.*—Study of the relation of motivation to memory has been strongly influenced by Freud's theory of repression. For more than 30 years psychologists have been seeking standardized laboratory methods for establishing the theoretically appropriate conditions under which a

subject's existing repressions could be measured or new ones created. By and large, these efforts have been unsuccessful; however, they have resulted in the addition of a considerable body of knowledge about memory.

Freud's suggestion that the meagerness of adult recall of early childhood experiences (infantile amnesia) is a product of repression has led to several studies of the character of such memories. Waldfogel (18) secured 85 min. of free written recall of experiences from the first eight years of life from 48 college men and 76 college women. The procedure was repeated 35 to 40 days later. The frequencies of recalled experiences, plotted according to the year to which they were attributed, gave an ogival distribution. Approximately half the memories recorded on the second occasion had not been given the first time, but the correlation between total number of events recalled on the two occasions was 0.70 for men and 0.76 for women; this substantiates earlier studies that have shown some individual consistency in the number of events recalled. What significance this fact may have is not clear; the method of counting "number of events" permits a good many other factors besides "ability to recall" to influence the results. Perhaps the most novel contribution of Waldfogel's study was his finding that an optimism/pessimism ratio based on the relative frequency of pleasant and unpleasant events recalled was somewhat consistent from one recall to the next; the correlation was 0.64 for men and 0.57 for women.

Another approach to the influence of motivation on recall grew out of Rosenzweig's studies (36, 37) of the effects of success and failure on the recall of completed and incompleted tasks. Rosenzweig conceived of the Zeigarnik effect (greater recallability of incompleted tasks) as a form of need-persistive reaction to frustration, i.e., a more or less involuntary effort to overcome the barriers preventing completion of a motivated act. He reasoned that under severe stress this type of reaction would be overcome by the ego-defensive process of repression, and that completed tasks (implying success) would be better recalled than incompleted ones (implying failure).

Attempts to test this hypothesis have produced contradictory results. Glixman (19) has shown that the confusion arises partly from a lack of clarity as to how the hypothesis should be tested. Both Rosenzweig and Alper (38), and more recently Sanford & Risser (20), used recall scores that involved subtraction of the per cent of incompleted tasks recalled from the per cent of completed tasks recalled. Glixman pointed out that the Rosenzweig hypothesis related to the differential effects, on the two types of recall, of varying degrees of frustration or stress, and that therefore the two scores must be kept separate in comparing recall between low and high stress conditions. He calculated the new indices from Rosenzweig's and Alper's original data and compared the findings with those from a similar study of his own (21). All three studies showed a decrease in recallability of incompleted tasks when amount of frustration (ego-threat, stress) was increased. This supports Rosenzweig's hypothesis. But

the three studies were contradictory with respect to recallability of completed tasks, two showing a decrease and one an increase. The significance of this diversity is not clear, and the complexity of the experimental situation created by "completion" makes speculation of doubtful value.

Sanford & Risser have reported a series of investigations that add further variations on this same theme. Unfortunately the authors reported most of their results only in terms of the type of scores properly criticized by Glixman. Two significant facts emerged, however, a comparison of the relative frequency of recall of completed and incompleted tasks showed that on puzzle tasks there was substantially less recall favoring the incomplete tasks than on a rhyming task which required the subject to give as many rhymes to a stimulus word as he could in a specified length of time. Continued thinking about the failed puzzle could not provide any ego-support, but continuing effort to find rhymes aided the subject in overcoming his failure feelings. This suggested to the authors that the ego-defensive reaction occurs only when it has some value in reducing the feelings of failure.

The other significant finding was that a second recall test, given after four months in two studies and after two weeks in a third one, showed less decrement from the first recall for the incompleted tasks than for the completed ones. On the first recall, all three groups showed higher recall for completed than for incompleted tasks (puzzles). At the second recall, completed and incompleted tasks were equally well recalled. Sanford & Risser interpreted this as evidence of a self-defensive forgetting process akin to repression, viewing the equivalent recallability on the second occasion as evidence that original fixation was equal and that passage of time had reduced the pain of the failure sufficiently to equalize recallability of the two kinds of task.

These studies emphasize the difficulties of using the "interrupted task" technique as a device for securing feelings of failure. The primary aim of the various researches by Rosenzweig, Alper, Glixman, and Sanford has been to investigate the influence of motivational strength or ego-pain on recall of the tasks with which the motivation or pain is associated. Yet this cumbersome method introduces an additional variable—the Zeigarnik effect—that is already known to influence recall, the very process that serves as the dependent variable in the experiments. Neither the logical implications nor the empirical effects of interruption are sufficiently known to permit any effective control of this extraneous but intrusive factor. Its only advantage is that it provides a means of making subjects fail. Since there are other methods of doing this, methods that do not activate any process already known to influence the dependent variable under study, there appears to be little reason for the continued use of the interruption technique. When a research operation requires as much discussion of its "psychological meaning" as interruption does, it is time to find a new operation.

This criticism applies to the use of interruption as a procedure for

securing failure reactions in the study of repression or other memorial functions. It does not necessarily apply to the use made of it in such a study as that of Alper (22), in which the subject's reactions to completed and incompleted tasks are used as measures in a battery of test situations for the discovery of trait congruences. Alper presented a complex series of stress situations to a group of 10 subjects who had been assessed in the Murray Diagnostic Council Experiment (D.C.E.). By an ingenious analysis of various performance characteristics as well as recall measures, she was able to define five major personality dimensions, or syndromes, in terms of the relative recallability of completed and incompleted tasks. Correlation of these measures with the D.C.E. ratings permitted the addition of descriptive categories of memorial behavior to syndromes previously established by other methods of personality study. In general, the syndromes were defined in terms of a subject's task-solution productivity and the social and emotional factors that influenced it.

*Level of Aspiration.*—It has been assumed for some years, on the basis of the work of Chapman & Volkmann (39), that the absolute height of a subject's level of aspiration was some function of his perception of his own ability. Gilinsky (23) verified this hypothesis by an experiment in which 10 groups of 10 subjects each were given an unfamiliar vocabulary test. Before the test was given, information was provided concerning mean IQ and the mean performance of a fictitious comparison group. The mean IQ's attributed to the fictitious comparison groups ranged from 80 to 170. The alleged mean vocabulary score was the same for all 10 groups. Each subject estimated his own IQ and stated his level of aspiration on the same scale as that the experimenter used for reporting the alleged mean performance of the subject's particular comparison group. The levels of aspiration of the experimental subjects were closely related to the perceived differences in ability between the individuals and their comparison groups.

This finding is of primary relevance to those situations in which a person is required to estimate his future performance without having had experience with the task in question. When he has had such experience, earlier studies have shown that there are marked and fairly stable individual differences in the size of discrepancy scores, and that these differences are related to differences in personal experiences of various kinds. The *d*-score is the difference between the previous level of performance and the aspiration level for the next performance. Further evidence is now available on the personality characteristics related to these differences. Escalona (24) compared the aspiration behavior of two groups of 19 high school students each who had been characterized as maladjusted and adjusted children respectively. Although the criteria for selection were poor, and the problem group was extremely heterogeneous in diagnostic categories, certain significant differences that were observed may suggest further analysis of this kind. Escalona found in the maladjusted group a greater rigidity of performance of the experimental puzzle-solving task,

more emotional expression when selecting a level of aspiration (together with greater denial of any feelings of emotion), a greater irrealism in the level, and a much slower decision time in making the choice. She found also that the maladjusted group more frequently ceased work on the experiment after failure experiences, while the adjusted group more frequently quit after having enjoyed success.

Sumner & Johnson (25) added further data on the d-scores of successful and unsuccessful students and analyzed their findings in terms of sex differences as well. The subjects were Negro college students and their levels of aspiration were set with reference to weekly quizzes in a psychology course. The successful students (upper quarter of class) had reliably smaller *d*-scores than the unsuccessful ones (lower quarter), which is in line with earlier studies. They also found that women had smaller *d*-scores than men when relative class success was held constant. They concluded that men are more expansive, daring, and less controlled by reality. This appears to coincide with the usual cultural finding that on the whole women tend to be more conservative, more attentive to reality, and more culture-bound.

A technical problem that has troubled students of aspiration for some time has been that of how to instruct a subject to give his level of aspiration. It has been suspected that the wording of the instructions might determine whether the subject expressed a wishful level or a practical estimate. Evidence that this suspicion is true has come in a compelling way from a study by Diggory (26). In a carefully designed experiment with 200 college students who were subjected to varying degrees of failure stimulation and then given two types of instructions, Diggory found that the average *d*-score for "hope" was about twice that for an instruction using the word "expect." Subjects were more self-consistent with respect to "hope" scores than "expect" scores. The size of *d*-scores varied inversely with seriousness of failure; the greater the failure, the smaller the *d*-score. This held true for both "hope" and "expect" aspiration instructions.

## Personality Structure

*Theory.*—Probably no field of psychology has been more perplexing to its students, with respect to theory, than has that of personality. The raw data of molar behavior provide such diversity that the psychologist can order his observations only by reducing the infinite multiplicity of human action to some schema containing a reasonably small number of variables. The situation is very different from that of learning, perception, or other segmental aspects of behavior. In these latter, the variables outside the process can be ignored, left to a colleague down the hall; there is no need to conceptualize total action systems. Personality, as a field of study, however, comprises just those totalities, and hence the theorist must devise a sufficient number of variables to provide a possibility of reasonably accurate analysis but not so many that he approximates the multiplicity of traits, motives, emotions, feelings, images, and thoughts

as these occur in the conscious or unconscious experience of the behaving person.

Any theory is valid only to the extent that it proves useful in predicting or providing for control of behavior; there is no right or wrong in the matter, but only convenience. Since no theory yet has proved brilliantly efficacious in ordering the data of molar behavior for these purposes, it is perhaps not surprising that so many psychologists find themselves goaded into new attempts to construct a systematic set of personality variables.

Duffy (27) has emphasized that the usefulness of such efforts is dependent on the degree to which the variables chosen are congruent with, or identical with, those used in other branches of psychology. She finds that the two basic dimensions of direction and intensity of behavior underlie all psychological systematization and proposes that these be the basic dimensions of personality theory. This approach, like that of other uni- or bidimensional theories, is of value to the extent that secondary principles of behavior are elucidated. These additional principles must relate to variables that can be operationally defined either in denotable events or by reference to the operation of other variables. It remains to be seen what kinds of specific principles Duffy will present; her two basic dimensions seem a sound foundation for their construction.

This does not appear to be the case with the "frame of reference" presented by Snygg & Combs (28), whose personalistic position seems to begin and end with the "discovery" of the self. Their enthusiasm over the fact that individuals differ from one another in their reactions to objectively described situations is difficult to understand. Essentially the authors were concerned to construct a system that would provide an understanding of the behavior of individuals rather than the changes in the mean behavior of groups. To this end, the self and its perceptions were taken as the basic data to be ordered. While there are numberless footnotes assuring the reader that "objective psychologists" (not cited by name) cannot predict the behavior of individuals, there is no evidence in the text that the authors possess any principles that will even predict the means of groups, let alone individual behavior. The major difficulty with the authors' effort to develop a useful system lies in their reliance on single rather than multiple variables for the analysis of given categories of action. The chapter on learning, for example, states in effect that all learning is to be understood as a process of differentiation. The laws of operation of the process of differentiation are not given, however, except by reference unsystematically to the common variables of learning that have been worked out on groups by "objective" psychologists. The level of analysis—and novelty—of the book is best exemplified by the following quotation (28, p. 50): "Perhaps the most important contribution of the personalistic point of view to the psychology of learning is the recognition that learning, like all other experiencing and behaving, is an active process which results from the efforts of the individual to satisfy his needs." Reinforcement theorists, and educational psychologists who

have been treading the Thorndikian path for 50 years, will doubtless receive this contribution with something less than maximum surprise.

*The Prejudiced Character.*—One of the most fascinating areas of study in the field of personality is the search for "characters," or syndromes of traits. These have value in that they may serve to reduce the total number of descriptive dimensions required for the ordering of behavior. If, for example, one could speak of an "oral character," and summarize thereby a host of interrelations among consistent behaviors, the task of describing people, or "personalities," would be much simpler than is at present the case. Further, the circumscribing of such behavior constellations would provide useful "consequent variables" for which life history or constitutional antecedents might profitably be sought. The study of oral character by Goldman (8), mentioned previously, is an example in point.

The results of a more elaborate approach to this kind of problem have recently been summarized by Frenkel-Brunswik (29). She and her associates at the Institute of Child Welfare, University of California, attempted to discover whether there are characteristic social, motivational, and cognitive factors associated with ethnic prejudice. Approximately 1,500 boys and girls, 11 to 16 years old, from varied socioeconomic backgrounds were given ethnic prejudice attitude scales; 120 of the extreme cases (from the upper and lower quartiles), and their parents, were then studied by interview. Analysis of the interviews provided the main data on motivational properties of the subjects, i.e., their character traits. Two technical papers by Frenkel-Brunswik (30, 31) describe the method of analysis.

The investigation revealed a fairly consistent set of correlates with prejudice. The prejudiced person was ethnocentric in the sense that he was not only opposed to other cultures, but attributed correspondingly high values to his own. He was aggressive not only toward minority groups, but toward everyone that had different ways or values from his own Second, he was hostile toward and contemptuous of anything that was weak. This was reflected not only in the negative and aggressive sense, but in the positive admiration and valuing of power and strength, as exemplified by money, masculinity, and age-status authority. Third, the ethnocentric was essentially a conformist, a moralist with absolute values. Fourth, he was intolerant of ambiguity and uncertainty in such social roles as those of parent-child, sex, and teacher-child. In an independent study, Rokeach (32) demonstrated that this rigidity extended even to the solution of intellectual problems. And finally, the prejudiced person saw the world as a dangerous place, full of chaos and unpredictability, with uncontrollable catastrophe ever-impending.

The exact amount of congruence between these characteristics will be assessable only with more extensive data, but Frenkel-Brunswik's comparisons of extreme groups provide a hopeful augury that a significant "character" may be well on the way to isolation.

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# SOCIAL PSYCHOLOGY AND GROUP PROCESSES[1]

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Social psychology during the past year, indeed in the period since the end of the War, has been engaged in catching up with itself. Provided with the opportunity for tremendous expansion in application during the war period—an opportunity mastered with imagination and daring, as Cartwright (1) has pointed out in his excellent if somewhat enthusiastic review of the war years—social psychology has been faced with several critical postwar problems. The least of these has been the problem of maintaining a high rate of production in the applied field. The problem, rather, has been a theoretical one, one which existed before the war but which did not become fully apparent until after the war when the imbalance of investigation and theory stood out in naked relief. The greatest problem which is faced by the social psychologist at the present time is the construction of an adequate theoretical model which can guide in the formation and selection of hypotheses worth testing. Without such a model or models, social psychology risks becoming a highly precise form either of tinkering or of social engineering. Our methods become increasingly exquisite; their use remains *ad hoc*.

It is against this somewhat forbidding background that one must evaluate the past year in social psychology. The year has been one of consolidation in the area of theory, of advance in methodology, and of a confused but prodigious growth in the understanding of social phenomena. Before turning to a critical evaluation of the literature which has appeared we attempt a brief characterization of some major trends.

## MAJOR TRENDS

(*a*) *The translation of traditional social psychological terms into basic psychological concepts.*—A rich history of contacts with other disciplines has left social psychology with a legacy of many phenotypic, *ad hoc* concepts whose major function was to restate what had been observed in a single word. Among the terms were imitation, suggestion, prestige, prejudice, and attitude. The past year has witnessed anew an attempt to deal with such concepts in terms of the basic psychology of motivation, perception, and learning.

(*b*) *Consolidation of social psychology and the psychology of personality.*—There is evident today an increasing smudging of the boundary between social psychology and the psychology of personality. Studies of attitude

[1] This review covers the period from approximately May, 1948 to June, 1949. The author wishes to express his gratitude to James Yandell and Donald Brown for their assistance in the preparation of this review.

and ethnic prejudice, such as those reported by Frenkel-Brunswik (2) at California, and Smith (3) at Harvard have dealt increasingly with predisposing personality factors. The provocative intrusion of psychoanalytically oriented anthropology has also had significance for the merging process. The area of communication research, with renewed emphasis being placed upon the "gratification study," has similarly reflected this trend.

(*c*) *Focus on the group as a unit of analysis.*—Fear of committing "the group fallacy," stimulated perhaps by an over-reaction in the 1920's to McDougall's *Group Mind*, has by now abated to the point where there is emerging in various research centers a sustained effort to study the structure of small groups, the communication process in groups, group productivity, and other phenomena referable not to individuals as such, but to the group as a unit.

(*d*) *Ferment in opinion-attitude methodology.*—In terms of sheer volume, opinion and attitude research continues to be the major area of research in social psychology. Although the prediction of elections has been a peripheral aspect of this work, the "failure of the polls" has hastened a trend toward self-criticism in methodology which has been in the making for several years.

(*e*) *Continued expansion of applied research.*—Research on group relations in industry, consumer wants and preferences, public reactions to policies and programs—these have become features of social technology in the United States. The range of application of social psychological techniques has been expanding beyond the borders of these traditional fields. One of the major projects of UNESCO (United Nations Educational Scientific and Cultural Organization) is the investigation on a world-wide basis of tensions affecting international understanding, progress reports on which have been provided by Cantril (4) and Klineberg (5).

(*f*) *The search for values.*—How to construct a social psychology which is both maximally useful in developing a democratic society and maximally scientific? The question as it has been phrased has had more than a metascientific significance, for in many instances psychologists have attempted to carry out what has come to be called "action research"—studies which, while seeking to achieve a socially useful end, are also devoted to the systematic collection of data and the testing of hypotheses. Perhaps Chein, Cook & Harding (6) put the matter most cogently. Remarking that the action researcher wishes both to be a good scientist and, at the same time, improve the world, they point out that from the very outset, the scientist impelled by the two motives faces special problems. He must formulate his research in such a way that its results are meaningfully applicable in the community in which it was conducted. His selection and use of investigatory procedures, the presentation of his findings, and the applications which he attempts are all geared to maximizing his chance of bringing about social advance in the community by his efforts.

## COGNITIVE PROCESSES IN SOCIAL BEHAVIOR

### Suggestion, Imitation, and Prestige

At least two major theoretical works during the past year have attempted somewhat related critiques of these three clasiscal doctrines of 19th century social psychology. One is Asch's critique (7) of experiments attempting to utilize these concepts in experiments on social judgment; the other is the systematic text by Krech & Crutchfield (8). Asch, strongly influenced by Wertheimer, examines several representative studies which attempt to show that judgment of social or esthetic issues is altered by suggestion. To quote Asch, the basic propositions of such studies are (*a*) "that an unchanged object of judgment undergoes change of evaluation," and (*b*) that "the material content of the object of judgment plays no role in the process of change." Asch's major thesis is that suggestion or prestige suggestion alters the entire cognitive context in which a judgment is made, the change in context being responsible, for the changed reasoning process.

Strongly influenced by the theories of Tolman, Lewin, and Köhler, Krech & Crutchfield bring to bear more directly on social psychology in the form of a series of propositions, the theoretical structure which has grown up in the fields of perception, learning, and motivation. Their treatment of suggestion, not unlike Asch's, stems from propositions concerning cognitive reorganization. There is no special response to stimulation which may be called suggestion. What has been called suggestion may be treated as a tension-reducing cognitive reorganization.

### Studies of Perception and Judgment

Several divergent studies during the past year have reemphasized that much of social behavior is attributable to the organization of the phenomenal field of the individual; and that cognitive organization is a partial function of needs, past experience, and internalized social standards. In essence, modern work in this area is a return to the early insistence of Thomas & Znaniecki (97) that the analysis of social behavior must begin with a study of how the individual defines the situation. The difference is that contemporary research is geared to investigating the determinants and mechanisms of socially relevant cognitive organization.

One group of studies is by Bruner, Postman, and their collaborators (9 to 11). Their work has been premised on the assumption that perception, as indeed all cognitive processes, must be regarded as an instrumental activity subserving the adjustive needs of the organism. In brief summary, these investigations have been concerned with the manner in which needs, internalized values, and various forms of stress affect both the relative saliency of objects and symbols in the perceptual field and also the readiness with which these objects and symbols are perceived. The studies have led to several tentative conclusions: (*a*) Personally relevant objects in the perceptual field, whether the relevance is a function of positive or negative

value to the individual, undergo accentuation, as measured in these experiments by an increase in apparent size. Certain reality demands, such as the need to use an object as an instrument or to have physical commerce with it, work against such accentuation. Carter & Schooler (12) have repeated an earlier experiment by Bruner & Goodman (98), which had shown that children accentuate the sizes of coins in relation to their value, and obtained negative results. When their subjects had the coins before them, there was no accentuation. Only when the sizes were estimated from memory did such accentuation occur. Further research is required to resolve the difference in results. (*b*) Perceptual selectivity, as measured by the determination of recognition thresholds in a tachistoscopic situation, is shown to favor the recognition of valued objects and objects associated with the prevailing interests of the subject. (*c*) Finally, under the pressure of threatening stimuli, the organism shifts from defensive avoidance of stimuli and becomes vigilant and perceptually selective toward that which threatens.

The work of McClelland & Atkinson (13, 14), while primarily designed to provide a basis in perception for certain apperceptive phenomena of the sort encountered in the TAT, has provided additional demonstration of the tendency for perceptual organization to be consonant with prevailing motivational states. Differentially food deprived sailors, candidates for submarine training, were presented stimulus situations of a highly ambiguous nature—dimly illuminated blank screens or smudged screens—and told that they were being tested for subliminal perception. There was a reliable tendency for the hungrier subjects to report more food related objects, these predominantly of an instrumental rather than goal related nature. But the more the structuring of the stimulus the smaller the number of food responses of the differently deprived groups. Where accentuation is concerned, the clarity, size, brightness, and numerousness of the food related objects reported tended to be more marked or accentuated for subjects who had undergone the greater deprivation. A repetition of the procedure using thematic pictures (14) to which subjects responded with stories showed no increase in food imagery or themas as a whole, but a selective increase in food deprivation situations, eating activity, and instrumental activity for coping with deprivation. In short, both at the fantasy level and in perceptual organization, increased need leads not only or not necessarily to gratification responses, but may lead instead to instrumentally-oriented activity. Additional experiments, utilizing as the independent variable differences in need for achievement, while considerably complicated by instructional problems, tend to confirm these conclusions (15).

Still another approach to a reformulation of social psychological principles in terms of the perceptual laws governing "definition of the situation" is that of Cantril, strongly influenced by the demonstrational experiments of Ames at the Hanover Institute. Since we shall discuss Cantril's application of his perceptual theory to the attitude process in a later section, it suffices briefly to outline some of the general axioms

from which he seeks to derive laws of social behavior (16). Perception is seen as purposive behavior, subserving the goal striving of the organism. On the basis of past experience, the individual learns how to weigh indications or cues in the environment and to perceive objects and events in relation to his past actions and present purposes.

From still another source has come the attempt to provide a link of continuity between social phenomena and basic cognitive processes. The California studies in ethnocentrism have provided at least two such linkages during the past year. One of them is in Rokeach's study of generalized mental rigidity and ethnocentrism (17). He set as his hypothesis that "one of the characteristics of ethnocentric thinking is a rigidity and inflexibility of the thinking process" which is "characteristic of the ethoncentric individual in his approach to all kinds of problems." He found the high in prejudice to be more concrete, less able to shift, less capable of verbalization of solutions in problem solving.

Frenkel-Brunswik (2), from whose work Rokeach derived his hypothesis, has similarly shown the manner in which prejudice and ethoncentrism reflect more general cognitive and motivational characteristics, among them an intolerance for cognitive ambiguity.

In studies on the perception of social and ethnic identity in the growing child Hartley and his collaborators (18) have described the manner in which role perception gradually shifts toward the use of ethnic frames of reference from earlier, more personalized ones. Merton (19) has been engaged in an intensive study of two housing projects and has shown how a resident's "image" or perception of the project is a case of "supplementary projection"—the attributes of one's own experience are projected onto the housing project as a whole.

Finally, a word about a closely reasoned paper by Helson (20), an attempt to rationalize the concept of frame of reference in terms of the theory of adaptation level. Although Helson's presentation is mainly based upon psychophysical phenomena, its relevance to such social phenomena as attitude and social judgment is underlined by the author. In very brief summary and without benefit of Helson's mathematical formulae, his position is this. Judgment of a stimulus is dependent upon the position of the stimulus with respect to the adaptation level of the organism. Adaptation level is a weighted measure of the center of the series of the stimuli which have been judged in a given situation. Whether a lifted weight or a social issue, judgment of the stimulus will reflect adaptation level. By supplying a provisory mathematical model for determining adaptation level for simple stimuli, Helson has given us a useful paradigm. Its utilization for social data will require the development of many constants the nature of which cannot be foreseen.

## CULTURE AND PERSONALITY

During the last year, two new anthologies dealing with the literature in the field of culture and personality have appeared: Kluckhohn & Murray (21) and Haring (22). Current research contributed by psychologists,

anthropologists, and psychiatrists appears on the increase. Views concerning the impact of culture on personality have grown in complexity. Some students in the field persist in assuming that early childhood disciplines as prescribed in a given culture determine adult character structure. Much is made, for example, of the importance of methods of infant swaddling in various European cultures as determinants of character structure in an article by Benedict (23).

A review article by Orlansky (24) utilizes available comparative data to test several hypotheses about the relationship of childhood experience and adult personality. An example of his procedure is the following. Permissive indulgence in early childhood, during the period of early discipline, is said to lead to a secure adult personality. Utilizing monographic material from as widely divergent groups as the Pueblo, Navaho, Kaska, Haitian, St. Thomas islanders, and various American ethnic groups, Orlansky is forced to reject such "simplicist theories." He would substitute a dispositional theory of the sort advanced by Leighton & Kluckhohn (25) which holds that early indulgence establishes a predisposition to security which actualizes if an adequate and appropriate cultural context is provided in later life. Orlansky's conclusions concerning the existence of invariant relationship between early childhood training and experience and adult personality structure are "largely negative." He calls for an increased emphasis on the study of "specific situations which can be calculated to produce specific psychological effects upon individuals with certain constitutions."

Sears and his collaborators (26), in work initiated at Iowa and continuing at Harvard, have approached the culture-personality problem in a somewhat different way, but again with the objective of more rigorous testing of hypotheses. Formulating the problem in terms of the concepts of social learning, Sears states:

> A society has verbal formulations of the way it solves many of its problems. The people who compose the society behave with varying degrees of conformity to these formulations. Children reared in a particular culture learn not only the verbal formulations but the necessary motivational systems that will ensure their conformity to the culture.

His research is limited to a few, highly significant subgroupings within the American culture: sex groupings and groupings according to ordinal position in the family. Surrounding both of these are elaborate rules of behavior which society attempts to inculcate. Aggressive and dependent behavior will be studied as the dependent variables. The mediating influences of varying parental treatment, personality ideals, and cultural patterns will be investigated in an effort to elucidate the process of socialization.

One final example serves to indicate the breadth of research stimulated by the "culture-personality approach." Ruesch, Jacobson & Loeb (27), concerned with the problem of acculturation and illness, have shown the

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way in which social mobility is associated with stress states which predispose to various illnesses. They point to the tension states of the social climber and its sequelae, the accident and injury proneness of the rebel, the relatively sounder adjustment of those who either do not choose to or are not forced to become mobile. The approach of these authors may be described as a study of the impact on personality of various typical cultural situations met by the horizontally or vertically mobile individual.

However one may criticize the present state of methodology in the field of culture-personality relationships, it is clear that the area represents a busy and fruitful channel of commerce through the usually high barrier which separates psychology from its close neighbor, anthropology. As Shils (28) remarks in concluding his provocative review of the present status of American sociology,

> To achieve the dignity of science involves not only an appreciation of and a skill in the use of techniques of accurate observation, recording, and codification, it requires also a sense of what is important.

Insofar as commerce with anthropology has stimulated in social psychology a sense of the importance of the psychological impact of major social institutions on personality development, it has enhanced our scientific dignity.[2]

## THE DESCRIPTION AND MEASUREMENT OF ATTITUDES

### Attitude Theory

It is perhaps in the area of attitude theory that one finds the greatest theoretical ferment in contemporary social psychology. Symptomatic of this are various attempts to reformulate the concept of attitude.

*Doob's reformulations.*—Doob, first in an article (29) and later in his *Public Opinion and Propaganda* (30), has attempted to do for the concept of attitude what his Yale colleagues, Miller & Dollard (99), did some years earlier for the concept of imitation. His effort to replace the concept with theorems coterminus with Hull's behavior theory is summarized in his own words: an attitude refers to

> (1) an implicit response (2) which is both (a) anticipatory and (b) mediating in reference to patterns of overt responses, (3) which is evoked (a) by a variety of stimulus patterns (b) as a result of previous learning or of gradients of generalization and discrimination, (4) which is itself cue- and drive-producing, (5) and which is considered socially significant in the individual's society.

Doob finds little utility in the concept "attitude" save as a heuristic device.

Perhaps Chein (31) has been foremost among Doob's critics. While

[2] Developments in the fields of sociology and anthropology cannot, because of space limits, be treated here. During the past year, however, Shils (28) has published an extensive and critical review of contemporary sociology to which the interested reader is referred.

questioning the consistency of Doob's formulation and Hull's learning theory—as for example in querying whether an attitude as a persistent implicit response is not a contradiction—Chein has also questioned whether Doob's theory can cope with some of the known complexities of attitude phenomena. The theory, he charges, cannot describe or explain the nature of an attitude as such, only how it is learned. It cannot deal with the cognitive-perceptual and other aspects of attitude, so central to the operation of the attitude process. His final conclusion is sharply stated: "It would be overhasty to abandon the concept of attitude on the basis of Doob's argument." The test, of course, will be whether Doob's views generate fruitful and testable hypotheses about attitude development as Hull's have about the learning process.

*Krech and Crutchfield's reformulation.*—Krech and Crutchfield (8) offer a reformulation which is less an attempt at reducing attitude to some other system of description than an attempt to show how cognitive and motivational concepts sharpen our understanding of attitudes when properly applied. An attitude is viewed as a mediating mechanism between fundamental psychological processes and action. As such, it may be looked upon as an enduring integration or patterning of motivational, affective, perceptual, and cognitive processes. An attitude is not a self-contained, self-starting mechanism, but is selectively brought into play by different kinds of environmental situations. Any given attitude performs an adjustive role in the process of need-tension reduction involved in adjustment.

*The Harvard reformulation.*—Growing out of the field and clinical studies on opinion toward Russia at the Harvard Psychological Clinic has come a reformulation in terms of the structural and functional characteristics of attitudes. An attitude is viewed as an aspect of the hierarchiacal structure of personality, being an expression of values which at a deeper level are dependent on the need structure of the individual. Smith (3) proposes that certain cognitive dimensions of an attitude or opinion can be described, such as informational level, and time perspective. Other affective dimensions include such variables as direction and intensity; cognitive-affective dimensions such as the relation of an attitude to other attitudes can also be distinguished. These represent the structure of an attitude. An attitude also serves certain intrapersonal functions: (a) reality adapting functions whereby an attitude serves to structure experience and aids in anticipating harms and benefits; (b) socially adapting functions leading to conformity or nonconformity of opinion depending upon the individual's social needs; and (c) ego-defensive functions through which the individual's attitudes aid him in coping with unacceptable or threatening needs and situations.

*Cantril's reformulation.*—From the demonstrational experiments on perception mentioned earlier (122–23), Cantril has attempted to infer a set of propositions about opinion change which he has applied to an impressive collection of trend figures on American opinion during the period

from 1939 to VJ Day (32). A brief statement of Cantril's twenty propositions follows. Opinion is formed in the face of frustration when a judgment is required. Based upon past experience, the opinion is formed on the basis of unconscious cues which are integrated when a value judgment must be made to support purposive action. Knowledge and thought serve to bring more cues to bear in this judgment process. Opinion changes when purposes change or when action—the final arbiter of the effectiveness of an opinion—demonstrates the opinion to be a poor guide to one's purposed goals. Specific opinions often contradict more abstract ones because the former may be action-tested, the latter not. Test-by-action may be vicarious in that we learn from watching others act upon opinions. A sense of surety characterizes action-tested opinions; ones not so tested often lack concreteness. Only personally relevant events affect opinion, and the intensity of an opinion reflects the importance of the conscious or unconscious purpose which it serves. Opinions involve the perception and judgment of other people and their reactions to oneself. "Rightness" of an opinion is a function of the range of relevant factors considered in arriving at it, and generally, over a period of history, opinions may be expected to grow more "right" as men learn through frustration and experience.

### Determinants of Opinion, Values, and Ideology

Two major studies have appeared during the past year, each raising important problems about and contributing materially to our knowledge about opinion determinants. One of them is the monumental study of soldier opinions during the last war (33) undertaken by an array of psychologists and sociologists under the direction of Stouffer. The other is a book incorporating and going beyond a series of articles by Centers (34) on the role of social class in the determination of opinion and ideology. In scope of content, methodological rigor, and daringness of conception, *The American Soldier* is one of the most comprehensive efforts yet undertaken by American social psychology. Much of the material reported is what the authors call "program testing" and is not designed to test basic hypotheses. Yet, from the work has come a set of theoretical and methodological contributions which demonstrate that at our present state of development, there are still many problems that can be tested in the setting of applied research. The work of Centers, while considerably more modest in scope, reopens various crucial questions about the nature of social class.

*Social status and its psychological consequences.*—For purposes of review we shall group here studies of "status endowing" characteristics as these affect opinions and values: income, occupation, education, social class as variously defined, military rank, and any other index which directly affects the person's place in the hierarchy of social privilege and respect.

The object of Centers' research, as he puts it in the introduction to his book (34), is "to analyze and interpret empirical [attitude] data gathered with specific reference to the problem of class consciousness. . . ."

Centers implies by class consciousness not objectively given status or status as defined by the community [as in Warner, Meeker & Eells (35)], but, rather, subjective status or class identification.

Concerning the origin of class consciousness, Centers proposes an interest group theory which holds "that a person's status and role with respect to the economic processes of society impose upon him certain attitudes, values, and interests relating to his role and status in the political and economic sphere." The individual then identifies with those who share his values—his social class. In support of his view, Centers points out that the four classes (upper, middle, working, and lower) with which his respondents identified tended to share common socio-economic attitudes. Moreover, when respondents are asked how they decide on whether a person is in their class, they name as most important how the person "feels and thinks about certain things." This emphasis upon shared beliefs, plus the communality of emphasis on proprietorship in the middle class and "working for a living" in the working class, is the brunt of Centers' documentation.

While Centers' thesis is provocative, his findings do not unambiguously prove his theory of the interest group. In the first place, there is a vast overlap in occupational position and attitudes of self-ascribed middle and working class respondents (the two major classes). It is not clear why, according to his theory, there are professionals who call themselves working class and skilled workers who call themselves middle class. It is quesable, perhaps, whether his respondents perceived the meaning of the four classes uniformly. Might it not have been more parsimonious to investigate first how class structure is viewed by different individuals and then to ask people with which of these subjective classes they identified? The use of fixed alternatives, it seems to the reviewer, begs the question.

Hollingshead's study of *Elmtown's Youth* (36) views the class-creating process at once more pluralistically and with less of theoretical commitment than Centers. He is less interested in the determinants of class consciousness than in how existing social stratification in Elmtown influences the behavior patterns of adolescents in the community. Analyzing clique formation, dating, the expression of aggression, and other everyday patterns, Hollingshead shows that there are large discrepancies in the behavior of the adolescents of his social classes I–V which can be traced to conformity pressures within the culture of the various classes. The crucial role of the family and of other institutions in transmitting behavior patterns is analyzed in considerable detail.

Perhaps Davis (37) has been most specific about the psychological techniques involved in the differential class training of children and adolescents. He stresses the greater emphasis on control of impulses, especially aggressive impulses, in the middle class family which is complicated at the same time by an ideal of virile masculinity.

From the studies of American soldiers during the Second World War (33) one may draw a considerable number of inferences about the effect

of social class position on attitudes, providing one may use educational level as a rough index of social class. Certain findings on job satisfaction in the Army and satisfaction with the chance to get ahead, the authors interpret by the concept of relative deprivation. Those with a higher level of attainment in civilian life feel more acutely the deprivations of the artificial Army situation because judgment of deprivation is made against their previous level of expectancy [cf. Helson's theory of adaptation level (20)]. Yet, in spite of a greater sense of deprivation, the better educated were the better adjusted to Army life. Stouffer and his associates show, however, that, in this respect, what is important is the family and community background of the various educational levels. For it is the better educated group that comes from the more stable families, that has made the best social adjustment in school and has had more dates with girls. And as might have been predicted from the studies already cited, the better educated group—more middle class?—were somewhat less likely than others to have liked fighting as youngsters.

Studies reported by Centers (38) and Centers & Cantril (39) shed interesting light on the concept of relative deprivation. Their objective was to find the degree of income satisfaction which exists at various income levels of a national sample. The findings indicate that, generally, the higher one goes in the economic scale, the greater the satisfaction with income. A parallel finding is that lower occupational strata are less satisfied than upper strata with the jobs they hold and, moreover, have lower job aspirations for their children. Given such a distribution of civilian satisfactions, the differential impact of Army deprivation on different levels is further elucidated.

*Information and enlightenment as determinants.*—Several studies emphasize the circularity of the relationship between liberalism and enlightenment. Schonbar (40), for example, indicates that in the area of opinions toward Russia, the attitudes of the college students she studied were more important determinants of how much information they acquired than vice versa. Her subjects, however, were too homogeneous a group to provide an adequate basis for generalizing these results. One may, moreover, question whether such results are fully meaningful without reference to the stage of opinion development of her subjects. Certainly at some stages of the growth of an opinion, information may be crucial in determining the growth of an attitude.

In one of a series of studies, Smith (41) has attempted to investigate the relationship of attitude, education, and information by a re-analysis of national opinion poll data. His findings may be summed up briefly. On issues which are interclass in nature where liberalism does not involve directly restriction of the more privileged segment of the population (e.g., internationalism, freedom of the press, etc.), those espousing liberal views tend to be the better educated and the better informed. On issues where liberalism involves restriction of the privileged class, holders of the conservative position are both better educated and better informed. The

conservatism of the well educated on economic issues is interpreted as a function of a feeling of threat, while the conservatism in matters of internationalism of the poorly educated is seen as stemming from a lack of information.

*Interaction of opinion determinants.*—One of the problems raised by the research of Lazarsfeld, Berelson & Gaudet (42) on political preferences in the 1940 election (the original monograph has been reissued during the past year with a new introduction) was the function of cross pressures on opinion. A cross pressure may be defined as the operation of two or more opinion determinants on the same individual or group, one determinant highly correlated with a positive opinion on an issue, the other with a negative. Kriesberg (43) reports a study of the opinions of members of a labor union local, an affiliate of a Communist dominated union strongly infiltrated at the local level by Communists, but containing a local membership predominantly Catholic in religious background. While his sample of cases is small and admittedly not typical of any general population, Kriesberg points out certain interesting trends in the opinions of his respondents on American-Soviet relations: (*a*) a minimum awareness of the existence of cross pressures, (*b*) a tendency to avoid cross-pressure situations, (*c*) a tendency toward moderation rather than all-out partisanship for an extreme among those aware of the cross pressures, and (*d*) a tendency twoard decline in interest associated with prolonged exposure to the pressures. Kriesberg's study is one of a few which seek to discover how cross pressures are experienced by the individual and how he reacts in concrete, cross-pressure situations.

It seems to the reviewer, finally, that the search for opinion determinants is not only becoming more functional (in the sense of inquiring into the function served by an opinion for the individual who holds it) but that also there is a growing realization that, when one discovers a demographic determinant of an opinion, it is then necessary to discover the kinds of psychological processes which are "packaged" in such labels as middle income group, urban, or middle aged.

## Opinion and Attitude Methodology

The volume of publication on opinion and attitude methodology was great during the past year, partly due to the general "tool sharpening" activity of opinion research and partly to the failure of the polls to predict the Presidential election of 1948.

*Interviewing problems.*—In introducing a symposium on interviewing problems, Harris & Connelly (44) remark that although interviewer bias and error perforce contaminate the reliability obtainable at all later stages of analysis of survey results, the task of interviewing in most research organizations is still entrusted to the lowest paid, least trained, and most impermanent members of the staff. Among the suggestions these authors make for reducing interviewer error are (*a*) use of Guttman scales to reduce error or at least make error determinable since such scales are less

dependent upon the single question, (*b*) increased interviewer training and participation [Borg (45) proposes the use of four-session interviewer schools to be set up by a research agency], (*c*) separation of the drudgery of enumeration from the task of interviewing, to be carried out by a separate staff in double enumeration areal sampling, (*d*) omission of costly and time-consuming call-backs and remote calls which add only a minimum to sampling precision, this perhaps to be accomplished by a modified quota-areal design, and, by implication, (*e*) the use of more permanent interviewing staffs.

In assessing interviewer bias, Hart (46) points to three characteristics of the interviewing situation which require further analysis: that it is a social artifact, a special dyadic relation which varies with a host of factors; that respondents vary widely in interpreting the situation and the questions asked; and that the interviewers also vary widely in their capacity to influence response. The main findings to date about interviewer bias are these. There is a positive correlation between interviewer and respondent opinions which is more marked in places of smaller population where the interviewer may serve the role of "conformity agent." Discrepancies in race and class between interviewer and respondent hamper rapport and may distort results in the direction of conformity. Interviewer bias results most when the interviewer has either a strong opinion on an issue or none at all. There is some evidence that part of the bias is in selecting respondents. Ambiguous answers to questions tend to elicit projection on the part of interviewers in classifying responses. And, finally, trained interviewers introduce as much bias as untrained. Hart reports that the National Opinion Research Center has undertaken a two year program of research on interviewer bias for the SSRC-NRC Committee on Measurement of Opinions, Attitudes, and Consumer Wants. The research, under the direction of Hyman, is undertaking the task of formulating and setting up experiments to test a series of hypotheses about bias.

*Sampling procedures.*—The controversy over probability sampling versus quota sampling has, in the opinion of the reviewer, been put into perspective in a judicious general survey of sampling problems presented by Stephan (47). Pointing to six peculiar difficulties involved in sampling human populations (profound regional differences, seasonal migration, daily movement of workers, reluctance to be interviewed, interviewer preferences for kinds of respondents, and differences in accessibility of population segments), Stephan points out how certain basic sampling principles are differentially affected by the use of quota, probability, and mixed sampling methods. Deciding which of the two basic approaches is better involves not only mathematical considerations—for in abstract terms, a perfect quota sample and a perfect area sample are Twiddle Dee and Twiddle Dum. Also to be considered are the behavior of interviewers and sample constructors, and the bank account of the sponsor. If in quota sampling there is a tendency for interviewers to avoid unsavory

respondents and run-down neighborhoods, there is also the problem, in probability sampling, of finding respondents at home and at the same time being able to complete a survey within the cost limits set. If quota sampling, as Blankenship points out (48), often leads to the social class upgrading of respondents, it is also true that the added travel involved in areal sampling requires a cut in the size of sample which can be covered on a given budget. As for the relative accuracy or agreement between the two approaches, there is still a paucity of carefully designed comparative studies, although several have recently been sponsored by the SSRC-NRC Committee on Measurement earlier referred to.

In conclusion, Stephan says:

> We have given considerable attention to the discussion of area and quota sampling. This issue should not be exaggerated as reflecting a deep and irreconcilable cleavage among persons engaged in sampling. The results of our studies tend to show that the differences between these methods of survey sampling are not absolute and that there are many effective variations of the methods that can be found between the two extremes. Their relative usefulness varies with the circumstances. Moreover, in the development of the fundamental theory of sampling, it appears that interviewers can be regarded as selecting devices or selectors, each of which may be represented by an operating characteristics curve with respect to the selection of persons with particular attributes in a population of a particular kind. This conception makes it possible to construct a probability model to represent quota sampling, and to that extent, provides a basis for theoretical comparisons, incorporating, if they are available, specific values of the fundamental parameters of the models.

Hochstim & Smith (49) have compared three methods of sampling (quota, block, and domal) with interviewers randomized and conclude that, generally, area samples yield better estimates of population characteristics than do quota samples. The less initiative in choosing respondents of the interviewer, the better the agreement with the Census figures. Manheimer & Hyman (50) point to several sources of error which can be made by interviewers in area sampling: errors in listing dwelling units, consisting of underlisting in poor areas; errors in following instructions to enter every *n*th household; and errors in enumerating and selecting respondents within households once these are chosen. The authors point out that it is erroneous to assume that area sample is completely independent of the selection performance of the interviewer and conclude that the theoretical requirements of area sampling must be translated into manipulable interviewer practices.

*Panel techniques.*[3]—Lazarsfeld (51), discussing the panel technique, enumerates several of its unique virtues. It allows one to locate opinion

[3] Unfortunately, the third volume of the soldier opinion studies (33) by Hovland, Lumsdaine, and Sheffield, was not available to the reviewer in time for inclusion in the present article. The principal technique used in the studies of mass communication there reported is the panel study or, basically, the classical four-fold "experimental-control: before-after" study.

changers and isolate their characteristics. Moreover, while a trend line may indicate no net change in opinion, panel study may reveal important shifts which balance out in the national total. One of the principal virtues of the panel is the opportunity it affords for studying the resolution of cross pressures on the opinions of respondents. The author also discusses critically some of the difficulties involved in panel studies: biasing by reinterview, particularly in the direction of stimulating respondent interest; the statistical and theoretical meaning of opinion turnover; and the expense and time involved. The interested reader will also find a discussion of panel problems in the introduction to the revised edition, Lazarsfeld *et al.*, *The People's Choice* (42).

*Question wording and scale construction.*[4]—The trend during the past year has been toward two types of inquiry. The first of these is toward an extension of scaling techniques; the second concerns the problems of intensive, open interviewing.

The basic problem which now faces those engaged in scale construction is either to find a general model which subsumes the varieties of scales which are now in use (the Guttman, Thurstone, Likert, Lazarsfeld, factor saturation approach, etc.) or, a second alternative, to isolate the different assumptions made by each approach. Work on this problem involving Stouffer, Lazarsfeld, Coombs, and Mosteller has been in progress at Harvard. Additional efforts have been sponsored by the SSRC-NRC Committee on Measurement of Attitudes, Opinions, and Consumer Wants.

It is quite evident that the rationalization of attitude measurement models is central for developments in social psychology. For each of the models makes assumptions about the nature of attitudes. The Guttman scale, for example, takes as an axiom that an attitude domain exists when and only when a sample of respondents arranges itself along a unidimensional scale pattern. The Thurstone equal-interval model makes a similar assumption, although it depends not upon the patterning of respondent responses, but upon agreement among a group of judges. Factorial studies share the same axiom. Yet, the basic axiom of the personologically oriented students of attitudes is that what matters is that there be an intra-individual unidimensional domain, whether or not this is shared by other individuals. One may argue, for example, that inverse factor analysis or a technique such as that used by Baldwin (52) is a more appropriate measurement model for approximating the usual definition of an individual attitude. Problems of this order, in the reviewer's estimate, must be faced if the present scaling models are to be optimally useful tools for aiding in the advance of psychological theory.

[4] The fourth volume of the soldier opinion (33) studies, the volume on measurement and prediction by Stouffer and associates, was not available for inclusion. The volume contains considerable discussion not only of the Guttman scale, but also of Lazarsfeld's latent attribute theory.

Methodological studies of intensive interviewing were few in number during the past year. What articles appeared stress the importance of getting at the latent meaning of attitudes, as for example, the critical essay of Riesman & Glazer (53) on what it means when people in different social classes hold and express opinions. They contend that in the upper and upper-middle classes, ideology is conscience directed and is strongly oriented toward action and responsibility. In the middle class, the opinion process, as measured in a polling situation, is a kind of "conversation between the classes" in which the middle class person, grabbing for straws of participation, vents his opinions and aggressions against the powerful classes and/or ruling individuals via the opinion poll. In the lower class—where the poll interviewer is usually middle class—the "meaning of having an opinion" is a matter of expressing an opinion in return for the rewarding rapport provided by the interviewer. Programmatically, their insistence is on a means of interpreting the latent meaning of opinion expression through relatively non-verbal projective means as well as through the usual interviewing procedures.

A projective approach to opinion measurement, one which is based on the assumption that judgments about the correctness of information will be influenced by the direction of one's attitude toward the information, has been presented by Hammond (54). Subjects are presented with an "information" test and asked to check which of two incorrect answers on each question is correct, each answer being intuitively relatively equidistant from the truth [e.g., Russia's removal of heavy industry from Austria was (*a*) legal (*b*) illegal]. Scoring is based upon the direction of the error-choice. Such error-choices differentiate between known groups. The author points to the utility of the technique as a means of getting around attitude test set and the kinds of conformity factors which operate in attitude test situations.

## VOTING BEHAVIOR

The "failure of the polls" in the Presidential election of 1948 had many consequences, not all of them grave. Whether the events of November, 1948 cut down the expansion of opinion research into new areas or seriously damaged the prestige of social psychology is something that cannot yet be judged. All that will concern us in this review are the reasons proposed to account for the failure and the lessons which have been drawn from it.

Shortly after the election, the SSRC appointed a Committee on Analysis of Pre-election Polls and Forecasts which examined the archives of the various agencies which had done studies or made predictions about the outcome of the voting. In brief, the Committee's conclusions (55) can be summarized under seven headings.

(*a*) The pollsters over-reached the capabilities of the poll as a predicting device. The election was so close that it is unlikely that there was justification on the part of polls to publish a flat prediction concerning outcome.

(*b*) More careful analysis of data would have revealed the closeness of the contest and more account could have been taken of the problems of a close election by careful consideration of past technical errors and by a special effort to detect late shifts.

(*c*) Making a prediction of voting behavior from poll responses involves eight steps, in any of which error may occur.[5] At two of these steps, serious error can be detected. The first is in sampling, in which there is to be found an over-representation of the better educated, allegedly justified by their over-representation in voting. But since an accurate prediction of the relationship between education and vote cannot be made, such an *a priori* sampling exclusion is not justified. A second source of error is inherent in methods of screening potential non-voters from the sample for purposes of vote prediction. Since there is no sound theory for predicting from verbal statements who will actually turn out to vote, error is inevitable. A second step at which error occurred was in forecasting. It consisted of a failure to assess properly the future vote of the undecided respondents and a failure to detect shifts in voting intention toward the end of the campaign. The undecided vote was great (approximately 15%), and it was allocated with insufficient justification to the different candidates in accordance with the proportions found among the decided voters. Postelection studies showed a marked shift to Truman or away from Dewey in the last fortnight of the campaign. Inadequate provision was made for the study of such shifts. Where data on trends were available, they were not properly assessed.

(*d*) The errors which resulted in the 1948 failure were the same as those which were known to have caused inaccuracies in 1940 and 1944.

(*e*) Reduction of error could have been effected by the utilization of techniques of interviewing and probability sampling now available to the pollsters. Reduction of error at other crucial steps must await a better psychological understanding of the relation between expressed intention to behave and actual behavior.

(*f*) The methods of publishing poll data used by the major organizations contributed materially to the widespread misinterpretation of and public reaction against the "failure of the polls." Predictions were made too flatly and inadequate information was given the public about the inaccuracy of the polling technique.

(*g*) The inadequacy of the 1948 prediction in no way reflects, in the opinion of the Committee, on the accuracy of properly conducted surveys which do not utilize inferences about overt behavior based upon expressed intention or opinion.

One intensive panel study, patterned along the lines of Lazarsfeld,

[5] Sampling design and instructions, questionnaire design and instruction of interviewers, selection of respondents, interviewing, deciding who will vote, deciding how to allocate the "no opinion" group, adjusting for trends and imbalances, forecasting and interpreting.

Berelson & Gaudet's study of Erie County (42), has been reported upon in preliminary form by Dinerman (56): a study of Elmira, New York, involving three pre-election and one postelection interviews of a panel of approximately a thousand respondents chosen by probability sampling. Results show that in the Elmira community, the greater part of the shift to Truman occurred between June and August and that the greatest source of late Truman strength was from amongst the undecided although some strength was gained by Truman directly from those who had been for Dewey. The main basis for shifting, as in the 1940 Erie County studies, was a function of class position—a return to Truman of those traditionally aligned with the Democratic party [cf. Lazarsfeld's Index of Political Predisposition (42)] perhaps as a result of an increasing awareness toward the end of the campaign of class-linked issues.

## ETHNIC PREJUDICE

Ethnocentrism and its determinants continue to be a major focus of research in social psychology. A brief review of the past year's work indicates that in this area perhaps more than in any other part of attitude research, the resources of many other parts of psychology as well as other social sciences have been brought to bear.

In general terms, research and discussion in the field of prejudice may be divided into four foci of inquiry: factors predisposing to prejudice, techniques for the reduction of prejudice, the effects of prejudice both on those against whom it is directed and on those who are prejudiced, and methods for the measurement of prejudice.

*Factors predisposing to prejudice.*—The trend during the past year has been toward the abandonment of the concept of prejudice as a phenomenon separate from other phenomena of intergroup relationships and attitudes. Wirth (57) points out that race relations are not different in kind from any other form of intergroup relations. Intergroup attitudes reflect such basic social and cultural processes as the loosening of local ties, the development of urbanism, mass migrations, the growth of mass communication, etc. in a society with characteristic patterns of stratification, traditions, and institutions.

Articles by Frenkel-Brunswik (2) and Rokeach (17) already cited underline the basic continuity of prejudice or ethnocentrism with more general personality characteristics. Frenkel-Brunswik shows the following to be more characteristic of highly ethnocentric than of tolerant children (ethnocentrism as measured by a modification of the California scale): less tendency to think of social problems in long range terms and more in terms of immediate welfare; greater projection of hostility onto outgroups; greater fear of and more rigid demarcation of other-sex behavior; greater respect for strength and greater hostility toward weakness; less companionable, more submissive attitude toward parents and authority figures; more exposure in home situation to emphasis on social status; greater moralism; less tolerance for ambiguity in perceptual and

cognitive tasks [it is here that Rokeach's findings (17) are most relevant]; more of a superstitious sense of catastrophic and chaotic forces in the world. The author points out that these are incipient patterns in the children studied (ages 11 to 16) which grow more marked with age.

At least one article during the past year has contained a thoughtful critique of the classical scapegoat theory of prejudice. Zawadski (58), criticizing first the "well-earned reputation" theory of prejudice as too purely stimulus-bound, then turns to the scapegoat theory as too simply based on drive theory. He points out that the scapegoat theory fails to account for the selectivity of prejudice against some but not other minorities, does not account for the respected position of some minorities, and cannot account for the prejudices of minorities against majorities. Granting the origin of the scapegoat theory as a reaction against racist doctrine, Zawadski notes the harmful effect of the doctrine on educational and policy planning. Group differences must be recognized; they exist even if they are not biological in nature. It should be recognized too that many undesirable traits in a minority are reactions against prejudice and can be unlearned under changed conditions. Finally, Zawadski calls for a convergence theory which takes into account objective factors of group difference as well as the role of drive transformation and projection.

Concerning sensitivity to the physical differentiae of minorities, Carter (59), repeating with variations an experiment earlier reported by Allport & Kramer (100), finds that there is no difference in the sensitivity of the anti-Semitically prejudiced and unprejudiced in recognizing Jewish faces. The previous study had found such a difference, using photographs of Jews and non-Jews of northern European descent. In Carter's variation, a third group of photographs was added: those of Mediterranean stock. Essentially, Carter's variation on the Allport-Kramer experiment sets the limits of the generalizability of the previous study. While in the earlier study the prejudiced group distinguished Jews from northern Europeans better than the unprejudiced, their capacity for correct discrimination may be thrown off by the inclusion of Mediterranean faces which may closely approximate the stereotype of the "Jewish face." The author suggests other possible reasons for the difference in results including the slightly greater prejudice of the Allport-Kramer subjects; the relatively low correlations between prejudice and sensitivity obtained by the previous study: and the possibility that some of Carter's prejudiced subjects showed the Allport-Kramer effect but that their contribution was washed out by grouping the data. Finally, Carter proposes to repeat the study using sound motion pictures in which more than the morphological characteristics of individuals of various ethnic descent can be portrayed.

*Methods for reducing prejudice.*—Wirth (57), describing methods of combating prejudice, lists these major approaches: (*a*) information, education, and propaganda, (*b*) facilitation of intergroup contact in various activities, (*c*) establishment of agencies to investigate and adjust inter-

racial differences and reward interracial understanding, (*d*) amelioration of groups and communities regarded as likely sources of tension, (*e*) organizations to combat undemocratic movements, (*f*) organizations to abolish segregation and discrimination in employment, housing, public services, etc., (*g*) civic organizations to combat unemployment and insure good government so as to minimize tensions leading to prejudice, (*h*) programs for training public officials in handling race relations, (*i*) psychotherapy and group therapy to modify prejudiced attitudes, (*j*) political pressure and legislation against unfair practices. Wirth remarks, as did Williams (60) in an earlier review of research on reducing intergroup tension, that we are still far from knowing the effects of these measures.

Two papers during the past year have been concerned with the effect of intergroup contact on the reduction of prejudice. McKenzie (61) studied the effect of various levels of contact between whites and Negroes. Perhaps the major finding of the study is that "acquaintance with Negroes of relatively high occupational status is an important factor in determining favorable attitudes of white persons toward Negroes." There is also some tendency for variety of contact to be associated with more favorable attitudes, although it is considerably less determinative than the occupational level of Negroes contacted.

In a study of Hogrefe, Evans & Chein (62), children who played at an interracial center showed no changes in overt behavior toward Negro children on the playground. Yet projective play techniques showed less tendency toward segregation behavior at the fantasy level than one found among members of a control group. Loomis & Schuler (63) have reported that contact with Americans may increase in Latin American students the sense of cultural differences between their culture and America's and may even strengthen certain damaging stereotypes rather than dispel them. The array of contradictory evidence on contact as a factor dispelling or increasing prejudice points, in the reviewer's opinion, to the psychological emptiness of the concept "contact"—what kind of contact, in what setting, etc.? Merton (19) shows in his study of two housing projects that one factor in bi-racial contact is to structure expectations about future contacts and thereby to allay anxiety. It is conceivable that, under certain conditions, the structuring might serve to raise anxiety.

The use of propaganda for reducing prejudice is discussed by Rose (64) who comes to the conclusion that, according to all theories of prejudice save the economic, propaganda should play some part in reducing prejudice. It may aid in rerouting the displacement of aggression, in undermining traditional prejudices, and in changing the social definition of groups against which aggressive urges have been directed. A review of past studies of propaganda effects show, in Rose's opinion, grounds for some optimism. Flowerman (65) takes rather strong exception to Rose's article, contending that unsupported pro-tolerance symbols in mass propaganda are not effective. His proposal, which in the opinion of the reviewer is supplementary to rather than in contradiction to Rose, is that

techniques for combating prejudice should be based on an understanding of the interpersonal situations and group needs which lead to prejudice.

*The effects of prejudice.*—Perhaps one of the most striking instances of the effects of prejudice on the segregated group is contained in the War Department studies of American soldier opinion (33). In a chapter on the Negro soldier, the authors show the extent to which Negro soldiers tended to define various universally stressful Army situations and difficulties as instances of racial discrimination. Among the Negro troops, there was also considerable cynicism about war aims and at the same time, a resentment about not being given a full chance to participate. It is interesting in the light of the theory of relative deprivation discussed earlier that although the Negro troops were aware of discrimination in Army job assignment, they were, at the same time, more satisfied with their Army jobs and re-enlisted over their quota at the end of the war. And, with reference to the problem of contact, excellent relations prevailed between Negro and white under combat conditions, but combat contact did not seem to have the effect of increasing the desire among white soldiers for off-duty, social contacts.

*Methods of measuring and assessing prejudice.*—Deri, Dinnerstein, Harding & Pepitone (66) have published an extensive and critical review of the literature on the diagnosis and measurement of intergroup attitudes and behavior. A very brief summary of their evaluation of various methods is the following: (*a*) ***observational studies*** have the virtue of directness and do not distort the behavior being studied, but they suffer from the difficulties involved in interpreting the rich complexity of that which has been observed; (*b*) the major virtue of ***written records*** is in providing access to the past, although certain forms of content analysis and personal document analysis may provide rich data about the person producing the record but little beyond that; (*c*) the ***study of specific intergroup behavior***, while directly relevant to assessing immediate action objectives, yields results difficult to generalize to other performances; (*d*) ***sociometric studies*** aimed at showing reduction in segregation often must contend with the confounding factor of familiarity within the group and must also solve the problem of what kinds of activity are most relevant to study by sociometric means; (*e*) the major drawback in the highly flexible methods of ***interviewing*** now available is in the injection of a confounding interpersonal relationship which is established in the interview situation [cf. Riesman & Glazer (53)]; (*f*) ***questionnaires***, a quick and easy method of obtaining a large amount of highly quantifiable material, have the disadvantage of being limited to those with higher literacy and, by and large, are not adequate to individual diagnosis; (*g*) ***pictorial techniques***, essentially a limiting modification of questionnaire methods, are useful only where the prejudice involved concerns groups with clearly visible differences but may serve to reduce conformity falsifications; (*h*) ***projective techniques*** are of greatest use when one is dealing with deep-seated attitudes of which the subject may be unaware. The interpretation

of results obtained is sufficiently difficult to warrant the use of a battery of projective tests.

Kramer (67) reports on the dimensions of prejudice, setting forth a set of ideal dimensions as a guide to the measurement of prejudice. He suggests that research be directed to the cognitive level of intergroup orientation, the emotional level, and the action level, and he assesses various forms of questions and scales in terms of these. He proposes that scales be devised for measuring various dimensions at each level. A virtue of Kramer's appeal is that, in setting forth an array of possible measurement dimensions, his paper may serve as a stimulus for consolidation of measurement techniques in the field of prejudice.

## THE STUDY OF GROUP DYNAMICS

Lewin's two early papers (68) on "Frontiers in Group Dynamics" are probably the outstanding programmatic papers in this field. The objectives and a summary of research of the Research Center for Group Dynamics at Michigan, strongly influenced by Lewin, have been succinctly summarized by Lippitt (69) in a paper at the Pittsburgh symposium on Current Trends in Social Psychology: (*a*) to apply experimental methods to the behavior of small groups and to the behavior of individuals in the group setting; (*b*) to attempt an integration in this work of the fields of individual psychology, social psychology, sociology, and social anthropology; (*c*) to isolate the comparative properties characteristic of small groups of different kinds operating under different conditions on different tasks and to attempt a translation of these properties into field theory; (*d*) to develop measurement applicable to small group behavior; and (*e*) to apply findings to such socially useful activity as supervisor training, leadership training, group therapy, etc. The work done by the Michigan group includes studies of communities, factory work groups, discussion groups, leadership groups in training, and other face-to-face groups.

One of the major problems investigated in group dynamics during the past year has to do with conditions leading to greater group productivity. Coch & French (70) have worked on the contribution of increased group participation in goal setting as a precondition of productivity and find that it succeeds by overcoming resistances stemming from competing demands in the situation. Again, in a series of papers edited by Bradford & French (71) on "The Dynamics of the Discussion Group" (a report on an intensive study of five leadership trainee groups at an isolated leadership training workshop) several writers report that the productivity of discussion increases as members come to accept the group goal and as their group roles increase in potency over competing roles. A corroborating study by Rosenthal & Cofer (72) shows the manner in which nonparticipation by one planted group member may cut down the aspiration and willingness to cooperate of the entire group.

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The understanding of differentiated roles within the group is another major focus of research. Benne & Sheats (73), writing on functional roles of group members, catalogue the various ways in which group members functioned in a group task: as initiators, evaluators, opinion or information givers or seekers, recorders, orienters, etc. Another set of roles has to do with group building and the establishment and perpetuation of group centered activity. The comprehensive list of categories to be used in connection with the continuous recording of group behavior reported by Bales & Gerbrands (74) is also concerned with the description of different role behaviors in a group. In a series of studies still in progress, Bales is working on the effect of different group structures on such role behavior as tension-reduction, suggestion giving, etc.

Another central emphasis in research on group dynamics is to show the impact on group behavior of the perceptions of group members. The primacy of the perceptual organization of group members was a major point in Lewin's programmatic papers (68). The "subjective frame of reference" in group analysis is evident in the Bradford & French volume (71) on the discussion group—even to the extent of utilizing a group thematic apperception test to discern the manner in which the group as a whole reacted subjectively to diagnostic material. It is significant too that one major part of the work done on the leadership training groups mentioned earlier was psychodiagnostic, an effort to discover the needs and subjective definitions of those participating in the groups being studied.

Research in group dynamics also centers upon the communication process within the group. The relation of communication to the maintenance of group productivity is constantly stressed. One of the important functions of the group leader, for example, is to feed back into the group communication which summarizes achievement and reemphasizes the group's objectives (71). Bavelas (75) has reported an interesting mathematical model of the group communication network showing how the existence of various kinds and patterns of communication pathways may determine group characteristics and even group leadership.

Cattell & Wispe (76), in a study of 21 groups each composed of six undergraduate women, have used an array of group performance scores on a variety of tasks as a means of discovering through factorial analysis the dimensions of small group syntality (syntality is to a small group as personality is to the individual). The dimensions which emerge are provocative: e.g., withdrawal versus responsiveness, intelligent *esprit de corps* versus internal friction. Their concern is with characterizing group performance taxonomically by the isolation of factors. Such an approach may prove useful to those working on observational studies of group process by providing a set of metrics for the codification of group behavior. At the moment, however, there appears to be a very wide gap between factorial studies and the more intuitive approach which now characterizes most of group dynamics.

## THE STUDY OF MEDIA OF COMMUNICATION

In an article on "Communication Research and the Social Psychologist," Lazarsfeld (77) restates Lasswell's traditional formula of "who says what, to whom, in what context, and to what effect" in terms of five dimensions: (*a*) control analysis—who determines the content of mass media; (*b*) content analysis—what is contained in the media and how may it be described; (*c*) audience studies—what are the media's audiences, how may they be measured and described; (*d*) effect studies—what is the effect of communication on an individual or audience and how to measure it; (*e*) functional analysis—what are the relationships of media to each other, and how may mass communication be related to the social system of a country and to other social phenomena.

The content of communication may be analyzed for various purposes. Thus, Berelson & Lazarsfeld (78), in the first monograph which attempts to canvass the field of content analysis, point to a host of varied uses including the following: to analyze values and attitudes in a culture, to predict events as in the wartime study of enemy broadcasts, to describe many varieties of trends over time in a given medium, to discover various stylistic, personal, and motivational characteristics of the originator of the communication, to discover the extent to which communication is living up to a standard, to assess the potential psychological stimulus value of a communication (e.g., its intrinsic interest or readability), and so on. The monograph describes past studies of a general and a methodological nature.

The past year has also seen the publication in collected form of the work of Lasswell's wartime group at the Library of Congress (79), a series of papers either previously published or widely distributed in mimeograph form. These studies, analyzing the attention focus in various countries of the world as these may be inferred from the press, and also concerned with the special analysis of various documents, are perhaps the most representative group of studies using content analysis methods stemming from or based upon Lasswell's system. One criticism may be ventured. Many of the categories used imply a probable effect of the communication on the audience, e.g., symbols of demand, balance of indulgence and deprivation, and such valuational frame of reference categories as the strength-weakness dimension. Yet to the best knowledge of the reviewer, no effect studies have been carried out to discover the probable impact (under different conditions) of communication so classified.

Three studies of content-as-reflecting-culture-patterns are worthy of special note. Analyzing the plots of popular dramas produced in Germany and the United States during 1909 to 1910 and during 1927, McGranahan & Wayne (80) use a thematic analysis in terms of which the basic dramatic conflict of the plays are categorized: love theme, morality theme, idealism theme, power theme, career theme, outcast theme. German plays were found to be more social minded, more philosophical, idealistic,

aggressive against superiors. American plays are more concerned with personal problems, are more moralistic, virtue tends more often to be rewarded, less emphasis is placed on power and ruthlessness as paths to success and more on conforming virtue, aggression is more often expressed toward parents. In German plays, rebellion is in the interest of an ideal, in American plays more often for the right to personal happiness. Insofar as popular drama is a reflector of a nation's projective systems, such a comparative analysis as this one should prove a useful tool for the cultural anthropologist concerned with the study of highly developed societies.

Johns-Heine & Gerth (81) have analyzed American popular magazine fiction from 1921 to 1940 in an effort to discern trends in the handling of major protagonists, settings, and plots. Although the statistical analysis in terms of decade periods is rough, certain trends are discernible. In terms of occupation, heroes are now less often in business or agriculture, more often professional men. Major protagonists are getting younger. There is a general trend away from rugged individualism and economic success toward idealization of the little man and the importance of group identification. Success through achievement is giving way to success through recognition and deference. For women, successful marriage and love are still major rewards.

Kracauer's analysis of communication trends (82) concerns the treatment of English and Russian characters in American fiction films since 1933. Kracauer concludes that the ratio of portrayal to projection is very low in Hollywood's treatment of these foreign groups. The ideals of relaxing entertainment and profit result in a lack of penetrating portrayal. Before the war, snobbishness and the caste system were the key foci in presenting British characters. During the war, humble virtue took their place. Since the war, perhaps because of the controversial nature of the Labor Government, Hollywood has left Britishers out of its plots. The pre-war treatment of Russian characters showed varying degrees of satire and aggression, which shifted to pro-Soviet material during the war. From 1945 to 1948, Russians were left out, in much the same way as Nazis were left out in the years immediately preceding 1939. Recently, Russians have returned as evil plotters. Throughout, in Kracauer's opinion, Russian characters have been more or less pure abstractions. The author contrasts Hollywood's stereotypy with the new trend in personalizing international understanding in Swiss and Italian films.

Perhaps the major audience study of the past year has been a monograph published by Lazarsfeld & Kendall (83) on radio listening in America, the second such study sponsored by the National Association of Broadcasters and carried out by the National Opinion Research Center. The study shows a general acceptance of American broadcasting although there is less satisfaction with radio than there had been two years earlier. In discussing the problems of improving radio, the authors make some succinct points concerning the fact that in radio and perhaps in other

media, supply creates demand. As in earlier studies, here too it is found that radio listening increases with decrease in educational level, and that where serious programs are concerned, they tend not to reach the less well educated listener.

Turning to the area of effect studies, controversies over readability of text and its effects continue. While Swanson (84) shows that news copy will be more thoroughly read when it is "more readable" as measured by various formulae [including Flesch's (101)], Kearl (85) points out that the major formulae leave out so much about content and organization as well as style that they perforce fall short of telling the whole story about readability. One phase of the effect of mass communication, generally overlooked, has been pointed out by Lazarsfeld & Merton (86): the growth of a sense of the ubiquity and power of mass media and their consequent role as setters of standards.

Wilson (87) reports on the CBS study of the effectiveness of seven documentaries put out over that network. The method used is to outline with the producer in advance the attitudinal and informational objectives of the program, and then run a before-and-after panel survey with questions directed toward these objectives. Effectiveness is measured in terms of change in relevant attitudes and information. Some of the programs were disappointing, e.g., one on incipient fascism; others were highly effective as, for example, one on the availability of medical care. The author grants the weakness of the method in not measuring the stability of change. A more serious criticism of this interesting approach to effectiveness is the failure of these studies to utilize a control group, particularly in the light of the findings of the War Department studies of mass media (33) indicating the presence of many factors producing change in a panel other than the communication under study. But the conception of the CBS approach, given the addition of a control panel, is a considerable improvement over past studies.

Festinger, Cartwright, and co-workers (88) report a study of the spread and distortion of communication (a rumor) through a low-rent housing project. The rumor, having to do with the alleged communist connections of a community worker trying to establish community activity which threatened to change the status pattern in the project, was found to spread as a function of the lack of cognitive clarity regarding the issue and of the perceived involvement and number of channels of communication of a potential hearer.

Warner & Henry (89) have attempted to analyze the symbolic content of a daytime serial (soap opera), examine the psychological and social characteristics of its audience, and study intensively the functions served by this serial in the lives of a small sample of 70 women who were listeners. Their monograph, an extension and theoretical refinement of the gratification study pioneered by Lazarsfeld, finds its conceptual basis in the sociology of Lloyd Warner and in Freudian psychodynamics. Its spirit is somewhat akin to Merton's earlier analysis of Kate Smith's war bond

marathon (90), a study rather oddly absent from the bibliography. The authors characterize the content of the serial studied as a secularized morality play emphasizing the values of middle-class life and the idealized middle-class family. By using upper-middle-class characters, the program stimulates emulatory reactions in an audience predominantly on the "Common Man social level." By emphasizing the importance of the role of wife and mother, it serves to allay incipient feelings of futility concerning this role which have become prevalent in our society. As in the case of McGranahan & Wayne's American dramas (80), these programs, too, are moralistic, portraying the victory and power of the virtuous over those who stray. In sum, the serial drama serves to reinforce middle-class values and to direct anxieties into socially acceptable middle-class channels. It is, perhaps, for these reasons that women in this and earlier studies have declared the programs "educational." Finally, the serial increases feelings of security by accentuating the stability both of marriage ties and of the breadwinner. It achieves the same end by reinforcing the superego in an implied condemnation of unregulated impulses. The study is noteworthy not only for drawing upon coordinate bodies of theory for its interpretation, but also for the use of projective techniques as a means of assessing deeper levels of gratification.

In a thoughtful, programmatic article on research frontiers in communication, Hovland (91) calls for the application of psychological techniques and theory to problems in mass communication. The Warner & Henry study (89), as well as others cited, is perhaps an example of the emergence of the field of communication as a major area of psychological research.

## SOCIAL PSYCHOLOGY AND WORLD AFFAIRS

The psychological problems of a period of armed truce have increasingly engaged the energies of social psychologists during the past year. In terms of research, the development of theory, and in establishing collaborative relations with students of international affairs, social psychology has taken important steps toward community responsibility.

One of the major developments is the establishment and activation of the UNESCO project on "Tensions Affecting International Understanding" at Paris operating on a world wide basis (92). The Project has as its general objective "to provide a means by which social scientists in all countries may contribute the knowledge and experience of their respective disciplines to the advancement of international understanding."

A brief summary of progress is provided by a UNESCO report of January, 1949 (92) and in an as yet unpublished presidential address to the Eastern Psychological Association by Otto Klineberg (5) in the spring of the same year. A first area of inquiry concerns "the distinctive character of various national cultures." A series of monographs is being prepared by groups affiliated with the International Studies Conference

on "ways of life" in seven countries. Studies of childhood ideals, community studies in several countries, analyses of the impact of legal systems—all of these are in progress. "Ideas which people of one nation hold concerning their own and other nations" represents a second research area. Samples in at least nine countries are being studied by opinion research techniques to implement work in this field; there are also studies of children's beliefs and the manner in which foreign nationalities are presented in mass media [cf. Kracauer (82)]. A third area concerns methods of changing attitudes. The Society for the Psychological Study of Social Issues (SPSSI) is preparing a handbook on this subject for UNESCO and memoranda have been prepared by various research centers. "Influences which make for international understanding or for aggressive nationalism" comprise the fourth main area of inquiry. A group of social scientists from various countries met in Paris to discuss this topic and agree upon a set of common principles (93). A book containing their discussion is now in preparation under the editorship of Cantril. The relation of population pressures and technological advance to international tensions are the last two foci of research of the Project.

In the late summer of 1949, a group of scientists, of which Rensis Likert was one, met in Paris under the joint auspices of UN and UNESCO to discuss the establishment of international research institutes. Among the institutes recommended is one concerned with research in the human sciences including social psychology.

Several studies during the past year have been concerned with problems of psychological warfare, a major weapon in belligerent international relations. Shils & Janowitz (94) evaluate the impact of Allied psychological warfare on the Wehrmacht, basing their inferences upon interrogations of prisoners of war. One of their major conclusions is that, insofar as the soldier's primary group is intact and effective in satisfying needs for affection and leadership, more abstract political appeals are relatively ineffective. Factors which weaken such primary group solidarity such as isolation, group heterogeneity, or the absence of a "father-child" atmosphere in officer-soldier relationships, increases desertion and psychological warfare vulnerability. It is interesting that of the Allied themes stressed, most war prisoners recalled those dealing with personal safety and good treatment, few recalled ideological appeals. Speier (95), in discussing the future of psychological warfare, raises the difficult problem of the objectives of such activity, pointing to the need for a political strategy which takes into account possible postwar relations rather than aiming only at quick victory. He stresses the importance of "judicious" rather than "moralistic" propaganda, the former stressing the common long-term objectives of the propagandist and the propagandized, Speier's review of past errors and future problems is a thoughtful assessment of a difficult problem.

The Bernays Atomic Energy Award of SPSSI was awarded in 1948 to Hornell Hart. A condensation of his prize study, "Social Science and the

Atomic Crisis" (96) traces the development of destructive techniques and outlines a "Manhattan Project of the Social Sciences" to be implemented by social scientists who, by objective means, are found to rank high in the scientific excellence and social significance of their published output.

## CONCLUSION

Social psychology has grown tremendously during the past decade. There is about it the vitality and sprawlingness of a new but unplanned industrial center. Much is being produced, but the relation of one product to another is often problematic, indeed puzzling. As yet, there are few areas in the field which are characterized by growth through orderly accretion. There are many and contradictory conceptual models in use, and it will be some time before the field has shaken down to the point where their relations to each other can be focused in an *experimentum crucis*. Yet if one compares the college-sophomore-oriented investigations of the years before World War II with the studies being published today, there can be little doubt that the growth has been not only in the direction of better methods, but also toward the consideration of many more and much more crucial variables. We may best conclude where we started: the critical shortage in social psychology is not in its lack of zeal for data but in its paucity of integrative theory.

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# INDUSTRIAL PSYCHOLOGY[1]

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A number of new textbooks in personnel and industrial psychology have appeared. In the general text by Ghiselli & Brown (1), the chapter on accidents is one of a number that is particularly complete. About half of Harrell's new book (2) is devoted to human relations, including psychological aspects of labor relations. Blum (3) emphasizes the social foundations of industrial psychology and includes chapters on consumer research, advertising, and selling.

A new book by Bellows (4) covers personnel psychology with emphasis on procedures and administration. Yoder (5) revised his book on personnel management as did Scott, Clothier & Spriegel (6). Both books make numerous references to psychological studies. *Personnel Psychology*, a new quarterly journal of applied research, made its appearance in 1948.

Thorndike's book, *Personnel Selection* (7), deals with the development and standardization of tests and test batteries; it includes a discussion not only of statistical procedures, but also of the problems which arise in administration of a testing program. A volume on testing by Lawshe (8) is less technical, having been written for use by persons in managerial positions as well as psychologists. A useful and comprehensive source book for the industrial psychologist on tests and test validity is that written from the standpoint of vocational counseling by Super (9).

## Broader Studies

A survey of the Aviation Psychology Program of the Army Air Forces by Flanagan (10) serves as a useful source book for general information about the program and a kind of index to the more detailed studies found in the other 18 volumes (See 17, 18, 117). According to Flanagan, the studies suggest that there are 50 to 100 independent traits and that these are important in classification and selection for jobs which also have a variety of specific characteristics or requirements. The determination of critical requirements is held to be the principal objective of job analysis procedures.

In a summary of the work of the Applied Psychology Panel of the National Defense Research Committee, Bray (11) described the administration of this wartime program and then discussed the several areas of research referring to the various technical reports. The projects were with the armed forces, principally the Navy; they included research on classification tests, on selection and training of specialists and communications personnel, and on human factors in fire control. Bray also

[1] This review covers the period from April, 1948 to July, 1949.

gave an outline of the future problems which military psychology may be called upon to solve. The report indicates that notable progress was made in studying the human factors in control of gunfire and also in formulating generalizations which should assist in future design of equipment.

Three of four volumes have been published describing the vast work in social psychology in the Army. Further mention is made of these studies under Motivation and Morale (90), and under Communications and Advertising (102). The Office of Strategic Services Assessment Staff (12) reported its wartime program of assessment of men for this important service. Many new test and interview procedures were applied. Although little validity was demonstrated, the program has important implications for industrial psychology, particularly in suggesting the use of techniques from social psychology.

From the foregoing reports as well as others from the armed services which have appeared since the war, there are four comments which might be made: (*a*) The studies, while utilizing various types of criteria, were short in combat criteria or even objective work performance criteria. (*b*) The studies failed to place sufficient emphasis on leadership and leadership jobs. (*c*) The studies have stimulated the development of psychology as applied to work environment and instrument design. New terms have been used to describe it such as "Engineering Psychology," "Applied Experimental Ps chology," and "Biomechanics." (*d*) The World War II psychology job was, in general, well done, and was well sold. The post war years have shown this in terms of the psychological work being supported by the various Departments of the National Military Establishment, and by increased work in industry.

A revision by the United States Employment Service (13) of the *Dictionary of Occupational Titles* contains a number of changes to facilitate its use, but few improvements in classification structure over the 1939 edition.

Wherry (14) demonstrated a method for showing on a factual basis the value of co-ordination among the parts of a personnel program. The study was conducted in 145 different civilian personnel offices of the War Department. Teams of field inspectors rated each office on its over-all effectiveness, its effectiveness in each of five major functions, and the adequacy of specific activities within each function. Intercorrelations among a number of variables were shown. The author concluded that for the situations covered, co-ordination of branch activities was as important, if not more important, than adequacy of branch staff itself in achieving over-all effectiveness in the personnel program. This study suggests the possibility of wider use of psychological methods in better understanding and evaluating administrative functions in industrial and governmental organization. Many psychologists in industry find themselves in administrative roles wherein techniques for evaluation should be useful.

### Interview

The interview, the most widely used personnel technique, received little attention in the literature. Uhrbrock (15) reviewed existing practices and listed descriptive statements which were developed in interviewing college graduates. He recommended that interviewers be trained, first, in conducting exit conferences; secondly, in making employee attitude surveys; and finally in the actual hiring of employees. Stenographic, phonographic, or dictated reports should be used as further training devices. In a historical review of research on the employment interview, Wagner (16) listed 96 different expressions that have been used in interview evaluation. Over-all fitness and physical appearance received the most tallies. Where available, validity and reliability data were reported. Attention was called to two trends in point of view toward the interview. The first emphasizes that more and more factors formerly measured in the interview can be better handled by other means. The second stresses that the interview can be valid if all information which can be obtained on the candidate is taken into consideration and weighted properly. He concluded that regardless of how validly or reliably a trait can be evaluated it can justify its inclusion in the interview only if it is related to success in the activity for which the applicant is being considered. Wagner's two trends raise important questions for interview practice and research. Can data be integrated better statistically than by human judgment? If prediction of over-all success is wanted and if a sufficient number of cases is available would not the objective integration be superior? If, on the other hand, the number of cases is limited would not integration by the interviewer appear wiser?

### Test Procedures and Norms

An important contribution to the psychological testing literature was the publication of the 19 reports describing the work of the Army Air Forces Aviation Psychology Program. Report No. 5 (17), covering the development of printed classification tests, is particularly resourceful. Job analysis data are presented as well as the analysis of test results. Factors and factor loadings are shown. Another valuable source book is Report No. 4 (18) describing the research on apparatus tests.

Noland & Bakke (19) made a study of employers' hiring requirements and sources of workers in two cities. Requirements were analyzed by occupational group. Formal tests of "aptitude and performance" were used by five per cent of the employers in one city and three per cent in another. The value of tests for selection in a tight labor market is often questioned because the job openings outnumber the applicants for employment. Selover & Vogel (20) demonstrated that in such a situation tests can be useful in assigning applicants to the jobs which best match their respective aptitudes. Data on four clerical groups were presented to demonstrate the process of "selective placement."

Stromberg (21) advanced the hypothesis that installation of a testing program by an employer results in a self-imposed selection whereby a greater proportion of the better qualified applicants seek jobs at the establishment. He presented data showing the superiority of the applicants to the criterion groups used in standardization. Stromberg's hypothesis was rejected by Macmillan & Roth (22) who suggested that test-taking motivation of applicants appears to account for the shifted distribution of their scores. They pointed out that added controls are needed for further experimentation and that different test examiners for the groups may be a factor.

In regard to efficiency of tests as selection devices, Jarrett (23) called attention to the desirability of describing such efficiency in terms of the ratio of the mean production of a selected group to the mean production of an unselected group. He presented a table from which the per cent improvement from selection may be estimated on the assumption of normality; to use the table, one must know the validity coefficient, the selection ratio, and the coefficient of variation of the unselected criterion scores. Using turnover rather than job proficiency as a criterion, Bolanovich (24) made an analysis of 271 interest items. Weights were assigned to 114 on the basis of their discriminating between "Terminators" and "Stayers." Straker (25) proposed that "where time allows, and where skilled interviewers are available in adequate numbers, the use of a rigid rejection level on psychological or educational tests is to be resisted." He was speaking of the selection of men for the British Navy by means of intelligence tests and an interview. Smith (26) warned that the Taylor-Russell tables are applicable only when the test scores and the criterion scores yield an approximate normal bivariate distribution which leads to an elliptical scattergram.

In a review of the professional literature, Ghiselli & Brown (27) located some 185 instances of reports of effectiveness of intelligence tests in selecting employees in various occupations. Distributions of the validity coefficients were presented for occupational groups, and the numbers of coefficients significant at the one per cent and five per cent levels were tabulated. In a further analysis Ghiselli (28) presented tables showing mean validity coefficients for 17 classifications of measures including personal data. Two criteria, proficiency and success in training, were presented. An annual or biennial accumulative summary such as this would be of considerable value to industrial psychologists.

Tiffin & Asher (29) reported norms for the Purdue Pegboard Test, a test intended for use in selecting employees for industrial jobs requiring manipulative dexterity. Validity coefficients for four jobs comprising 14 samples were given. A short test of mental ability called the Classification Test for Industrial and Office Personnel was described by Otis & Chesler (30). An attempt was made to include items of approximately uniform difficulty and to keep the level of difficulty low. This is contrasted with the usual practice of progressively increasing the difficulty of the items with the result that a subject may "get stuck" on an item.

Brown (31), in discussing the industrial uses of the Rorschach, predicted increased use of it which will lead to confusion because of improper application by untrained persons. He also warned that "statisticians in the field of psychological meaurement will apply the usual techniques of validation to the Rorschach and will come to the conclusion that it is not internally consistent or valid" and that one should be cautioned against accepting such statistical analyses at face value, since their application to personality tests in general and the Rorschach in particular is open to question. Richardson (32) criticized Brown and indicated that the Rorschach must be required to stand up under the same rigorous standards as other tests.

*Supervisory and professional personnel.*—Interest has developed in the use of observations of group situations as selection devices for jobs in industry, particularly for supervisory positions. This interest has been stimulated by the war experiences abroad and in this country. To date, practically no validation has been reported. Taft (33) employed a role-playing game, leaderless discussion, and problem situation discussion, but did not report the relationships obtained among the variables.

Guilford & Comrey (34) used a 150 question mulitple-choice type personal-history inventory in a study of 300 school principals and vice-principals. The authors concluded that the biographical-data method has only limited promise of usefulness for the selection of school administrators. However, it may have been that there was sufficient variability in the work patterns of the administrators so that the subjects should not have been grouped in one occupational sample.

Henry & Gardner (35) using projective tests, described personality evaluation methods in the selection of executives and Henry (36)described "the personality communalities of a group of successful business executives." From the standpoint of the validation methods subscribed to by this reviewer, the results were not convincing.

In a survey of supervisors in 52 organizations, Eisenberg (37) obtained rankings of the qualities they considered important for first line supervisors and for co-ordinating supervisors. Skill in training subordinates ranked high for first line supervisors and skill in selecting personnel was high for the second group. Richardson (38) developed scoring weights for Bernreuter items which distinguish between office-holders and non-office-holders and between supervisors and non-supervisors. He did not consider the weighted items suitable for selection purpose although a second sample was utilized. The Adaptability Test was used by Lawshe (39) in two studies of supervisors. Those who were no longer supervising tended to score lower than those who retained their supervisory roles.

Mandell & Adams (40) made a preliminary report of their work in developing methods for selecting scientists. Of 16 tests, including a biographical data blank, nine showed encouraging results for certain occupational groups. Subjective criteria were used.

A study by Abt (41) of 66 technical magazine editors in a large publishing firm used a criterion representing 23 aspects of work and per-

sonality. The Personnel Classification Test and the Michigan Vocabulary Profile Test showed significant differences between the criterion groups as did work sample tests on a total score basis. Results from the Bernreuter Personality Inventory "suggested that there were qualitative differences between individual editors in the two criterion groups," but no statistically significant differences were shown.

*Sales and clerical personnel.*—Reviewing techniques of salesmen selection since 1935, Husband (42) was rather pessimistic concerning tests because of the importance of a social or personality function in selling and the imperfections of personality tests. He was more optimistic concerning the Strong Vocational Interest Blank, and believed that interview procedures have not done too well although McMurry's standardized interview shows promise. He indicated that clinical analysis was valuable for validative purposes and that personal data were promising. In a review of sales personnel selection from 1935 to 1945, Cleveland (43) found the problem of selection pretty well defined but the ultimate solutions obscure. He believes emphasis will continue on (*a*) standardized and evaluated application blanks; (*b*) "diagnostic" or "patterned" interviews; and (*c*) standardized tests and inventories. Many of his 156 references were not from the professional psychological literature.

A study of 84 new life insurance salesmen who had attended a life insurance marketing course at a university is reported by Kahn & Hadley (44). They collected data on selling activities, such as size of application written, number of calls made, number asked to buy, and number of applications written. Personal data items and a battery of tests were administered to the group. The criterion of success was based on the total of signed applications. The upper and lower thirds were designated as the two criterion groups. Although the degree of success during approximately the first three months showed significant relationship with later selling, the predictors did not show appreciable correlations with the criterion.

Kurtz (45) found that the Rorschach Test failed to predict the success or failure of life insurance managers. The scoring method was developed on a first group of good and poor managers but when applied to a second sample the test did not predict. Review of the work of several other investigators indicated that the Rorschach, both group and individual test, shows generally negative results in personnel measurement. Cox (46) administered the Harrower-Erickson Multiple Choice Rorschach Test to department store sales personnel. A scoring key was developed from item analysis, the criterion groups being the upper and the lower 25 per cent on the basis of subjective rankings. A number of items appeared significant but the test was not tried on a second sample. Sinaiko (47) gave the Rosenzweig Picture-Frustration (P-F) Study to 53 department store section managers. Quantitative measures of job proficiency from personnel review data were correlated with the six prnicipal scores derived from the P-F Study. Statistically significant negative relationships occurred between the criterion and scores for Extra-punitiveness and for

Ego-Defensive. Dougan, Schiff & Welch (48) tried out the Reorganization Test on department store employees in a display department. A contingency coefficient of 0.60 was obtained between the manager's ratings of originality and the test scores. Professional artists scored significantly higher than store display personnel, but the latter group did not have scores significantly different from those of unselected students.

For selecting tabulating equipment operator-trainees, Taylor & Tajen (49) tried out a battery of tests including clerical speed, word meaning, arithmetic figure cancellation, and figure classification. Items of personal data including age, sex, highest school grade completed, were also included. Alphabetical and numerical grades in the training course were the criteria. The clerical speed and the figure classification tests gave consistently the best results. The letter grades showed quite consistently closer relationship to the tests than did the numerical (objective) scores. A stenographic proficiency test was described by Seashore & Bennett (50). The material (letters) to be transcribed is presented on standard phonograph records instead of being dictated by an examiner.

Giese & Weigle (51) evaluated the StenoGaugE and the Clerical Test D which had been given over a period of about eight years to nearly all applicants for clerical and stenographic positions. The correlation of StenoGaugE scores on 43 cases with supervisors' ratings was 0.61. The Clerical Test D on 72 cases correlated 0.39 with supervisors' ratings but failed to differentiate upper from lower groups. It was concluded that the StenoGuagE test should be retained but that the Clerical Test D should be replaced. This study is one of few in which the selection device was evaluated by using test scores obtained at time of employment.

*Pilots.*—An important study (52) of the relationship between visual ability and flight performance was completed by the Committee on Aviation Psychology, National Research Council. The study represented co-ordinated efforts of psychologists, ophthalmologists, physiologists, and others. One hundred and ninety-four males were carefully selected by visual and other tests for four criterion groups. Those of Group A had "normal" vision without glasses; Group B, essentially "normal" vision provided that correcting glasses were worn; Group C, considerable visual deficiency, not necessarily correctible to normal by the use of glasses; and Group D, effectively monocular. Thirteen criterion variables were studied and learning curves were shown in terms of several of these variables. The study suggested that a male representative of the C or D group has about five chances in ten of successfully completing the course and a man with normal visual efficiency approximately 8.5 chances in ten.

A cross validation study was reported by Finesinger, Cobb, Chapple & Brazier (53) on naval flight training. A variety of measures including psychiatric interviews, measures from the "Interaction Chronograph," various psychological measures, the abbreviated Rorschach Test, and the Thematic Picture Test were administered. Multiple correlations between

certain of the predictors and a criterion of flight training success were between 0.75 and 0.85. On a second sample the predictors were of little value.

Gordon (54) analyzed the personnel records of 432 pilots from five major airline companies. The records, including test scores of those who had been released because of lack of flying proficiency, were compared with those currently employed. Gordon also studied the job of airline pilot from the standpoint of critical requirements. From an analysis of records and from interviews with pilots and check pilots, 21 critical requirements were listed. The findings suggested improvements in flight examinations, cockpit design, training programs, and methods of selection. From the standpoint of selection, this approach to job analysis appears to hold considerable promise for it yields important data often overlooked by psychologists whose chief interest is the application of tests for selection.

*Mechanical and other personnel.*—A study by Rundquist & Bittner (55) was concerned with foremen's ratings of glass-container selectors. The foremen rated the workers on ability to do the job, personality characteristics suitable for the job and over-all rating. They concluded that inconsistencies of relationships between tests and ratings may be due to inconsistencies of the ratings rather than any deficiency of the personnel device.

Beamer, Edmonson & Strother (56) studied 29 linotype trainees. Using a combined speed and error criterion they developed a multiple regression equation involving the Schrammel-Brannon revision of Army Alpha, the Kuder Preference Record, the Minnesota Vocational Test for clerical workers, the MacQuarrie Mechanical Aptitude Test, and the Revised Minnesota Paper Form Board. The weighted battery correlated 0.82 with the criterion.

A multiple choice farm knowledge test was described by Gregg (57). The purpose of the test was to discriminate persons with farm background from those with no farm experience. The biserial correlation was 0.73. Kerr (58) administered a battery of vision tests to 226 female respondents employed as radio tube workers. The test scores correlated moderately low with efficiency ratings and with experience.

Wylie (59) published the results of testing 55 men who entered a Navy diesel engine school. The Grove Modification of the Kent-Shakow Industrial Formboard Series correlated 0.47 with final grades. The test correlated from 0.5 to 0.6 with so called mechanical aptitude tests and between 0.2 and 0.3 with general ability tests.

## Job Analysis and Evaluation

The book by Otis & Leukart (60) on job evaluation appears to be the best over-all treatise on the subject yet published. Although more attention is given to point systems than to factor comparison, the authors are

not attempting to sell a system, something unusual for a job evaluation book.

The method of paired comparisons and a specification scoring key was used by Satter (61) in evaluation of jobs. Using the first method, jobs were analyzed in two plants by methods modeled after those used by the United States Employment Service. Job descriptions were prepared for use by the judges. Key jobs were set up to reduce the number of comparisons which were made on four factors or "traits." A scale was prepared for each factor. Going pay rates were used as criteria. Employing the second method, Satter used a form somewhat similar to the Worker Characteristics Form of the United States Employment Service. The ratings for the 18 items were made "on a co-operative basis by the job analyst, the immediate supervisor and by the employee preforming the job." The paired comparison method seems preferable when it is desirable to have a number of judges involved and when a minimum of training can be given the judges. The other method was recommended in situations wherein a relatively small number of new jobs needs to be fitted into an established wage structure or when the small number of jobs makes a simpler, shorter procedure necessary. Satter's study is commendable in that he cross-validated and also because he used a wage criterion rather than a total job score. Psychologists seem to be wary of using wage data; yet, in job evaluation, it is the pay going with the job that really counts.

Two re-evaluations of the National Metal Trades Association method of job evaluation were carried out by Cohen (62), using the same job descriptions as in the original evaluation. In the first instance the re-evaluation correlated 0.949 with the original point values. The second re-evaluation correlated 0.951 with the original. The physical demand factor was the least reliable, and responsibility for safety the most reliable. Chesler (63) had job analysts rate 35 jobs using a 12 point system, then six of the analysts rated the jobs according to the system used by his particular company. The reliability of ratings with the 12 point system was high and intercorrelations among the six different company systems ranged from 0.89 to 0.97. The six systems included two factor comparison methods, one ranking system, and three point systems. Lawshe & Farbro (64), using their abbreviated rating system found reliabilities of management committee members to be consistently higher than those of labor for 43 jobs. High correlation was found between individual ratings and those adjusted by the group in conference. Ash (65) also investigated reliability in job evaluation. He used nine factors selected from a number of systems. Twenty-seven composite occupational descriptions were ranked by 10 job analysts according to each factor. Highest average reliability coefficients were as follows: for "adaptability," 0.93; "working conditions," 0.91; and "knowledge," 0.89. The lowest coefficients were: "attention," 0.39; "physical effort," 0.64; and "responsibility for work of others," 0.64. In the foregoing studies, the reliabilities were in terms of

ratings of already prepared descriptions. Studies in which the entire job analysis and evaluation porcess is performed by different analysts are needed.

In the seventh in a series of job evaluation studies, Lawshe, Dudek & Wilson (66) had analysts rate job descriptions according to the National Electrical Manufacturers Association System and a simplified system. Five factors were found which seemed to account for the elements in the two systems. The authors did not imply that the five factors would account completly for job evaluation of all types of jobs, but they did suggest that, for each family or group of similar occupations, a core of items designed to measure the five factors might be used in setting up a system.

In a study by Chesler (67) raters from three companies evaluated a standard set of job descriptions using the standard manual of 12 factors. The Wherry-Doolittle method of weighting variables was applied to each of the three sets of ratings. The results substantiated other findings to the effect that technically the briefer scale does as well as the longer one. Chesler (68) also used the Wherry-Doolittle selection method in determining factors for predicting total ratings made with the use of three different company manuals. Hay (69) presented a "per cent method" for developing factor comparison key scales. The ranks of factors are converted to percentage relationships by judges. The application of Weber's law was pointed out and adjustments made accordingly. Turner (70) also described a "per cent method" of job evaluation based on an earlier article.

Bellows & Estep (71) applied the Occupational Characteristics Check List (OCCL) of the United States Employment Service to evaluate office jobs. When compared with the use of a two factor system the OCCL was quicker to apply and showed promise of future development. Bass (72) used addends in building a systematic classification of clerical and sales occupations. Although the technique was employed as an aid in vocational counseling, it tends to be useful in job classification. The addend code indicates certain patterns of job characteristics.

Stogdill & Shartle (73) reported on the use of scales for measuring responsibility, authority, and delegation as practiced by persons in administrative roles. Relationships were shown with level in the organization and with various work patterns. Shartle (74) described the use of modified job analysis techniques including sociometry for studying executive positions, a part of a 10 year research program on leadership in industry, government, and education. Fourteen classifications of executive activities were defined and patterns of executive performance and staff behavior were illustrated. Hypotheses relative to leadership were presented.

The effectiveness of the small group situation for obtaining written responses was determined by Wagner (75). The personnel information sought was descriptions of effective and ineffective behavior observed in Air Force officers. The results of the group technique were compared with the individual interview method. The group interview accomplished

the work in one fourth the time with no loss of quality and no adverse morale effects.

### Criteria of Performance

Sisson (76) gave a further description of the Army forced choice method of rating which was introduced for use in 1947. This method involves the use of selected pairs of phrases or adjectives, such that they appear of equal value to the rater but differ in their significance for success as an officer. The pairs are assembled into tetrads and the item weights are determined by comparison with an external subjective criterion. Raters do not know the weights. This method of rating is characterized by less influence of officer rank, better distribution of rating scores, and more objectivity. Such rating techniques do not permit the rater to know how high or how low his rating is actually scored. While this has a technical advantage, it raises the administration question as to the advisabiliy of having a rating system which does not permit the rater to know how he has rated his subordinates.

The development of procedures for flight check for the Air Transport Rating was reported by Gordon (77). The standards for this criterion of flight performance included (*a*) reliability, (*b*) measurement of relevant skills to success or failure, (*c*) practicability in application, and (*d*) acceptability to those using it. Methods were devised to measure the critical requirements. These included check list items which applied to preparation for flight, equipment familiarization, taxiing, and so on. In the first trial, the reliabilities for observer-observer was 0.85, and for ride-ride, 0.58. Revision of the scale included altering items of low reliability and having more pictorial and graphic items. This brought the ride-ride reliability up to 0.76. Nagay (78) used job analysis [critical requirements approach originally described by Flanagan (10)] to develop criteria for evaluating the proficiency of air route traffic controllers. He studied the presently available measures of controller proficiency. These included civil service ratings, performance ratings, certificate examination, hours of flying time, and years of experience. Intercorrelations were shown but not reliabilities of the variables. The job analyses showed the proportion of time spent on activities and the critical incidents. A procedure for evaluating proficiency by using statements of critical behavior was developed.

Ferguson (79) made a study of the value of the acquaintance of raters with the subjects being rated. The basic data were 3,029 ratings of acquaintance assigned to 1,597 assistant managers (ratees) by 104 traveling field representatives (raters) in an insurance company. Weights were assigned to degree of acquaintance upon the basis of distribution of three degrees of acquaintance. Greater degree of acquaintance was associated with higher reliability of ratings and higher appraisal validity (relation of ratings to records of performance). Mahler (80) compared experimentally two methods of rating employees. The currently used scale was revised

by increasing the number of degrees from four to five and by making the descriptive phrases more definite. Two factors were added and the form was rearranged to facilitate the ratings of all workers (sales clerks) upon one trait at a time. The new scale showed wider distribution and less skewness but no significant increase in reliability.

Lawshe, Kephart & McCormick (81) described the Personnel Comparison System for rating job performance by the paired comparison method. The names are arranged in booklet form for ready use. A method of scaling paired comparison ratings so that new cases might be inserted within an already established scale without a new rescaling of all items was proposed by Angoff (82) .

## Training

An analysis was made by McGehee (83) of the relationships between production at various learning periods for 21 textile operators. Correlations were obtained between production of each 40 hr. period with every other 40 hr. period; biserial r's between production periods of fast and slow learners and time to reach average production; and multiple correlations between production periods and time required to reach average production. Fast and slow learners could be differentiated as early as the end of the second week and separation or transfers could be made with 20 per cent better than chance at this early date. The method used is adapted to small samples which are common in industrial training studies.

King (84) gave a rather complete description of the development of a training program for hosiery linkers in which the psychologist learned the tasks and therefore had first-hand experience with perceptual, social, and other factors: a method little practiced by most industrial psychologists, but one which should get more attention. The problem of training was considered by Haire (85) as an example of line-staff relationships. He looked upon training as a line responsibility, with the staff expert as a helper and adviser. Learning was viewed as a reorganization of the environment, and the environment chiefly controlled by line management. The foreman may learn a new principle in a training conference but if the total work environment remains the same the new principle will probably not be applied. Haire's point of view is one that has recieved too little attention in proportion to the attention given specific training devices and teaching methods. Webb (86) reported the use of a check list of training difficulties which was administered to fixed gunnery trainees after each gunnery mission. The items checked were compared with gunnery scores. The results suggested critical areas of training which deserved particular attention in the training program.

Mold (87) outlined the objectives of an executive development program, listed the qualifications of trainees, and outlined the steps in training. Although the report is primarily an outline lacking in detail, psychologists interested in the subject should get ideas for such a program from this article. No steps are suggested for the validation of the techniques

used although considerable interview, test data, and other facts were obtained. Thompson (88) presented a book on personnel management for supervisors.

The File-Remmers *How to Supervise* questionnaire and ratings by trainees were used by Katzell (89) to evaluate an eight weeks' training program in human relations for 60 supervisors in a large railroad. The results generally supported the conclusion that the training program was useful.

### Motivation and Morale

Stouffer *et al.* (90) reported on the extensive attitude studies made in the army during World War II. Men who said they did not get a square deal in the Army gave as their major complaint their assignment to branch or job. Attitudes toward leadership, combat, and Army practices were analyzed. Criteria were introduced, such as combat performance, to test the validity of the survey findings. The use of objective criteria represents a very encouraging forward step in attitude test measurement.

The Survey Research Center, University of Michigan (91), is engaged in a ten-year program to discover the causal conditions making for high level group performance and morale as well as satisfaction of individual members; also, the program is attempting to determine specific techniques for applying the general principles of group motivation and morale to particular situations. In an insurance company, first line supervisors in high production work-groups were less closely supervised, placed less direct emphasis on production as a goal, encouraged employee participation in making decisions, and spent more of their time in supervision and less in straight production work. Groups with higher production showed greater pride in their own work-groups. The findings suggested that persons are more effectively motivated when they are given some degree of freedom in the way in which they do their work. Such studies raise interesting questions as to what is cause and what is effect. For example, a unit which is doing well might quite naturally receive less detailed supervision. Did the kind of supervision produce the result or did the behavior of the group produce the type of supervision, or was there no causal relationship at all? The Surevy Research Center has further studies in progress which should throw light on many of these intricate problems.

A study of work adjustment in relation to family background was described by Friend & Haggard (92). Data were obtained by extensive interviews at a counseling service. The results supplied much information relative to the apparent relationship of early family life to work satisfactions. The results suggested that "the worker appears to compromise with life by going after the identical and specific satisfaction in work denied him years before. The over-dominated boy often strives to be completely free from supervision on the job." Centers (93) found, in a nationwide sample, 87 per cent of white collar workers satisfied with their jobs, as compared to 78 per cent of manual workers and 95 per cent

of farm owners and managers. Low pay or poor profits was given most frequently as the reason for dissatisfaction with insecurity ranking second.

A promising approach to the study of industrial conflict is that of Stagner (94). He believes that from the psychologist's point of view there are two sets of factors which require attention: (*a*) the purposes or motives of the individual (the needs he is trying to satisfy in a given situation), and (*b*) the way in which he perceives that situation. Four factors affect perception: namely, (*a*) biological quality, (*b*) past history of the person, (*c*) the purposes of the individual, and (*d*) attitudes. Stagner illustrates how industrial executives and union leaders differ with respect to the latter three of those four factors and hence how they may be expected to differ in their perceptions.

Over 3,000 applicants for positions in a public utility ranked 10 defined factors in order of importance in a study by Jurgenson (95). Men applicants rated the factors of greatest importance in the following order: security, type of work, and advancement. Company, pay, co-workers, and supervisor were intermediate. Hours, working conditions, and benefits were least important. For women the items considered most important were the same as for men, but the rank order was type or work, security, and advancement. Supervisor, company, co-workers, working conditions, hours, and pay were given intermediate ranks and benefits were rated lowest. This study illustrates the importance of a firm knowing what its applicants look for in employment, a type of information which needs to be further explored by industrial psychologists, particularly in relation to changes in labor market conditions.

Campbell (96) studied the attitudes of maintenance employees in a large manufacturing firm. Preliminary interviews were conducted on five per cent of the employees to provide the general and specific areas of employee satisfaction. Questions were then prepared for two parallel questionnaires. Follow-up interviews were conducted on a second sample of five per cent of employees to obtain a more meaningful interpretation of the analysis of questionnaire responses. In developing a quantitative morale scale Harris (97) treated responses to an industrial employee survey according to test development prodecures. Items were rated by judges for appropriateness and those for which there was 80 per cent agreement were selected. The 100 highest scoring employees and the 100 lowest scoring employees were selected, and the degree of internal consistency of each item was determined in terms of discrimination or D-values. Items with D-values of 1.0 or better were arbitrarily retained. The 36 item scale had a corrected reliability of 0.84.

Eckerman (98) analyzed grievance data in a large midwestern plant. The most frequent grievance pertained to pay and wages (30 per cent), the next largest concerned jobs and work (28 per cent), with seniority coming third (10 per cent). The grievers had worked longer for the company than the non-grievers and had accumulated more seniority. Other differences were pointed out but one wonders if it was not length of serv-

ice that "made the difference" rather than being a griever or non-griever. The interrelations of the variables should have been investigated to see if correction for length of service or age were necessary.

Kerr (99) developed a method for validating the Tear Ballot. He reasoned that past job satisfaction was a suitable criterion and that a valid measure of past job satisfaction was past tenure. A job tenure rate was computed from data received with the ballot which was administered anonymously. The rate was computed by dividing the wage earner's total time in civilian labor market by the number of jobs held during that time. Seven of the 10 principal items in the ballot correlated significantly at the one per cent level with this criterion. The highest coefficient was between tenure rate and opinion of work associates, suggesting the importance of strong interpersonal ties in preventing employee separation. This study has suggested an approach which is certainly worth further investigation; in the job situation there is great need to explore criteria for a better understanding of the meaning of job satisfaction. Hayworth & Nolan (100) outlined a mental health program for employees using group discussion methods. A review by Hand, Hoppock & Zlatchin (101) of job satisfaction researches for 1944 and 1945 covered 34 reports.

### Communications and Advertising

Hovland, Lumsdaine & Sheffield (102) reported the extensive experiments on mass communications conducted in the Army during World War II. Orientation, training, and general interest films were evaluated by using as principal criteria information gained and opinion change. An "effectiveness index" consisting of the ratio of change obtained to maximum change possible was developed. Audience participation and giving "both sides" of an issue enhanced change. Useful principles which have bearing on training effectiveness, attitude change, and motivation were discussed. Hovland (103) also outlined the general research needs in communications.

A new readability yardstick by Flesch (104) involves a revision of his formula which was published in 1943 and based on a count of average sentence length in words, number of affixes, and number of references to people. Flesch indicated that this formula did not always show high readability of direct conversational writing, and that the scoring system was sometimes difficult to use. The new procedure has two formulae: one for predicting "reading ease," and the other for predicting "human interest." Steps in applying the formulae are given with a sample of reading material. The "reading ease" score is based upon average sentence length in words and the syllables per 100 words. The human interest score is based upon the number of personal words per 100 words and the number of personal sentences per 100 sentences. Tables for rapidly converting the readability raw data into index numbers were prepared by Farr & Jenkins (105).

Perloff (106) studied 190 articles in a weekly magazine and developed

a regression equation for predicting starting readership per cent, based on the number of men who, in a survey, reported that they saw the article and started to read it. The multiple regression equation yielded a correlation coefficient of 0.56. Variables in the order of importance were (*a*) classification of subject matter, (*b*) number of illustrations, (*c*) sex of persons in illustrations, (*d*) color of illustrations, and (*e*) proportion of text devoted to opening page(s). Perloff (107) also predicted female readership with a correlation coefficient of 0.70 for one sample and 0.73 between actual and predicted on a cross validity sample (a very unusual finding). The order of relative importance of the five factors changed for the women subjects.

The effects of true altitude from 5,000 to 35,000 feet above sea level on speech intelligibility were investigated by Kryter (108). Anoxia was prevented by the use of oxygen equipment. An articulation test of monosyllabic words was applied. Intelligibility decreased during flight from about 65 per cent words correctly heard at 5,000 feet to near 40 per cent at 35,000 feet and was attributed to a depression in operating efficiency of the voice, microphone, and earphone. Additional interphone amplifier gains are suggested for high altitudes.

Clark (109) made a study of the attitudes of residents toward an "industrial nuisance": namely, an oil company which stores and distributes fuel oil. A sample of residents in the neighborhood was selected by interviewing one person from every third household listed in the city directory. A majority of those interviewed held opinions unfavorable to expansion or entrenchment of the plant. Those living nearer the plant when compared to those living farther away showed little difference in attitude toward plant expansion and the presence of the plant in their own property values; but they showed more evidence of fear and anxiety regarding threat to safety. Evans & Laseau (110) introduced the General Motors' "My Job Contest" as an example of communications in industry.

An experiment by Harrell, Brown & Schramm (111) was concerned with memory for radio news broadcasts in a laboratory situation. As would be expected, human interest and spectacular stories of crime and disaster were remembered better than stories of public affairs, but repetition of facts within a newscast did not have a significant effect. The effects of the rapidly growing industry of television were investigated from the standpoint of leisure time activities by Coffin (112). He used matched families and personal interviews. Families were classified according to socio-economic classes. The television families showed a considerably lower level of participation in other activities and were enthusiastic about the medium. It was concluded that television may bring about appreciable changes in the family's pattern of leisure time activities.

The psychogalvanic response was used by Eckstrand & Gilliland (113) as a measure for predicting the effectiveness of advertising material. The problem was the relationship between the total arousal produced by the ad and the ad's sales effectiveness. In one series of ads where effectiveness

of ad A was 2.1 times as great as either of the other two ads B and C, both Chi square method and $t$ test indicated ad A "better" in terms of psychogalvanic response at the 7 per cent level of significance. In two other series significant differences were not found in galvanic response but the differences in criteria were less pronounced. The authors consider the method worthy of further study and point out the advantage of the objectivity of the psychogalvanic measure. In two studies on the identification of cola beverages by taste, Bowles & Pronko (114) found that identification was not significantly better than chance. In a study of inexpensive and expensive perfumes by Locke & Grimm (115), untrained judges differentiated the two groups slightly better than chance. Errors were in the direction of selecting expensive perfume as being inexpensive.

## Human Engineering

Excluding problems of human relations, personnel management, or labor relations, Mead (116) defined human engineering as "that area of scientific endeavor which seeks an optimal rapprochement between the human individual and the mechanized tasks which he is required to perform in our society." He listed as characteristic of human engineering three types of experimental studies: optimal environment, equipment controls, and man-machine systems. According to Fitts (117) the purpose of engineering psychology is "to collect data on psychological capacities and limitations of individuals and to apply these data to engineering design problems to the end that the equipment which is finally produced will be adapted to the requirements of the man who is to use it."

Christensen (118) described a method of activity analysis applied to arctic aerial navigation, using sampling intervals. Ellson (119) discussed the application of operational analysis which he described as a "method of analyzing input-output functions of transmission systems designed to make possible the prediction of out-puts where the input is known." Gilbarg presented its mathematical basis.

Payne-Scott (120) presented a mathematical formula for the visibility of small echoes on radar plan-position indicator displays. Grether (121) reveiwed recent studies in design of instrument dials referring to recent government reports. Grether (122) also carried out an experiment to study the design factors which are related to the speed and accuracy of reading clocks in the 2,400 hr. system. The 24 hr. dial was slightly superior to the 12 hr. dial and dials with numerals on the minute scale were superior to dials without such numerals. The subjects were 62 rated Army Air Force Officers and 100 high school students. Using as a criterion legibility measured in terms of accuracy for brief readings, Sleight (123) found significant differences in accuracy of reading for five types of instruments. A preliminary study of aircraft instrument displays for use with the omnidirectional range was made by Williams & Roscoe (124). They found "pictorial" displays superior to "symbol" displays but errors were made with both to the extent that further research on errors was suggested.

Roscoe (125) reported a study of airplane pilot landing performance under the following conditions of visibility: (*a*) outside vision restricted to an image on a ground glass screen by a projection periscope; (*b*) similar restriction of outside field by use of vision reducing goggles and a vision directing screen; and (*c*) unrestricted visibility as in normal contact flying. In (*c*) binocular cues of depth and movement were eliminated, but they were retained in (*a*) and (*b*). The pilots made safe approaches and landings in all three conditions by using whatever cues were available. Landings in (*c*) situation were significantly more accurate than in (*a*) and (*b*). There was some suggestion that peripheral binocular cues were relatively more important than the peripheral monocular cues but the results were not conclusive. In a further report concerning his studies of stalls, Rulon (126) concluded that stall recognition is specific to the flight condition in which it is attempted and pilots good at stall approaches in one condition of flight are not necessarily good in other conditions.

Sleight & Tiffin (127) reviewed the problem of noise in industry and summarized some of the recent studies including hearing loss of shipyard workers and of boilermakers. The authors concluded that the greater quantity of experimental evidence indicates that there are many circumstances wherein noise is deleterious. In a factory where noise appears to be a problem the authors suggested three initial steps: (*a*) measurement of the noise in terms of intensity level and composition, (*b*) institution of feasible noise reduction and elimination measures, and (*c*) the establishment of a hearing testing program particularly for pre-employment testing.

Little outside of textbooks was published regarding accidents. Mintz & Blum (128) re-examined the accident proneness concept. They pointed out that in many instances the portion of variance in accident records attributable to differences in all forms of accident liability (personal characteristics of people contributing to the likelihood of their having accidents) "is relatively small as compared to the residual variance attributable to the operation of factors which are not predictable in terms of either the constant characteristics of people or of their previous accident records." In the data they examined the variance attributable to differences in accident liability was in most cases between 20 and 40 per cent.

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# ANIMAL AND PHYSIOLOGICAL PSYCHOLOGY[1]

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The subject matter of this chapter does not really form a separate department of psychology. Physiological and animal psychologists are concerned with the same problems and the same theories as other psychologists. Physiological psychology is distinctive, however, in its use of a particular kind of method and in its conviction that when one has postulated some process intervening between sensory stimulation and the consequent behavior (to permit theory to deal with facts that would otherwise be discrepant with it), one has the scientific obligation to subject the hypothesis to every feasible test, and to examine the properties of "intervening variables" as directly as possible. Otherwise, error may often be needlessly perpetuated.

Animal psychology likewise is not a theoretically distinctive zone, but comparative psychology is distinctive in much the same way that physiological psychology is; and it is comparative psychology that is discussed in the present chapter. Animal psychology is the psychology of learning, of perception, of emotion, even of thought, and its peculiarity is often only in the use of co-operative subjects that remain conveniently in cages and can be found when they are needed.

## Comparative Studies

The comparative method, theoretically, is analogous to the physiological method, as a correlation of structure with function. Since evolution has provided animals with different structures, neural, sensory, or motor, one may proceed experimentally to discover what features of behavior vary with changes in a particular structure (especially in the central nervous system), just as one may modify structure by surgical operation within a single species and observe the correlated changes in behavior. It has proved difficult in practice to make much use of the comparative method, because one usually cannot separate out one structural variable while controlling others; the effect of size of cerebrum per se, for example, is not known because any great increase in the cerebrum is also accompanied by complex changes in sensory and motor structures. Few exact studies of this kind have ever been made, but occasional contributions have been of great value.

Another approach, in which the comparison is implicit but still important, involves the study of lower animals in which behavior is (presumptively) simpler. This includes the study of "instinctive" behavior, and the history of psychology repeatedly shows that higher behavior patterns

[1] This review covers the period from January, 1948 to June, 1949.

have been better understood as the result of study at a lower level. It is unquestionable that our understanding of man in general has been profoundly influenced by comparisons of this kind.

Schneirla (1) has provided an excellent statement of the problems, present status, and general methods of comparative psychology. The problem of the comparative study of intelligence has also been reviewed by McBride and the writer (2) in a study of the captive porpoises of the Marine Studios. The porpoise, *Tursiops truncatus*, has a very large cerebrum and cerebral cortex, and the brain of a related species, *Delphinus delphis*, has according to Grünthal (3) some strikingly primate characteristics, with a very interesting parallel in two independently developing phylogenetic lines. In view of the size and structure of the porpoise brain, and the unanimous conviction of the caretakers of *Tursiops* that he is very "intelligent," it was of considerable interest to ask how intelligent he is in comparison with other species. The question, however, simply cannot be answered in objective terms. There is at present no good way of comparing the intelligence of disparate species. The aquatic porpoise, as limbless as a fish, and, like a fish or a bird, not confined to solid surfaces, poses the problem in an acute form and draws attention to the possibility that the chimpanzee's reputation as an animal genius depends to a considerable degree on his bodily similarity to man and particularly on his possession of the anthropoid hand, rather than to a highly developed brain.

The positive side of this study (2), however, is an attempt to outline a comparative psychology of emotion and motivation, suggesting that here might be found a better basis for arriving at a psychological hierarchy among animals: in the type and duration of self-maintained play, in fear of innocuous stationary objects, sexual deviations from the biologically adequate pattern of copulation, and so on. The argument depends in part on the significant comparative analysis by Beach (4) of sex behavior. Beach concludes that as the cerebral cortex assumes an increasing control of behavior the direct influence of hormonal factors in sex behavior decreases, at the same time that the range of exciting stimuli and the variability of response increase. It seems at least as possible to define the distinctive characteristics of "higher" sex behavior as those of higher problem-solving. The definition is in rather general terms, but at least it does not beg the question. In other words, sex behavior is a useful index of evolutionary status in the behavioral sense.

Edinger (5) has given an important reminder that existing animal forms do not make up an evolutionary scale and cannot be ranked simply as more or less evolved. There is no linear path of psychological ascent or increased complexity of behavior to be traced among these living forms that is identical with degree of evolution. The horse has been evolving as long as man or monkey. We cannot simply class all invertebrates as psychologically (or behaviorally) more primitive than all vertebrates, or

class rodents in general as lower than carnivores, or carnivores lower than primates. Though it is evident that the behavior of some species is much higher or more complex than that of others, we cannot decide which is higher by reference to supposed phylogenetic status, but must learn to solve this psychological problem by psychological means. When we do, we shall almost certainly find that the conception of a general level of intelligence is useless and that we shall have to learn how to define, not a single continuum, but a number of continua, requiring qualitative as well as quantitative analysis of behavior.

The study of Gardner & Nissen (6) has given evidence anew that we cannot use the deceptively simple solution of rate of learning as an index of animal intelligence. The subjects were (*a*) horses, cows, and sheep, as a "domestic animal" group; (*b*) chimpanzees; and (*c*) human idiots and imbeciles, the "ament" group. The task required, to discriminate one food box distinguished by a black cloth from two others, was about equally suited to all subjects, that is, differences of motor dexterity and sensory acuity would not significantly affect the results. No clear hierarchy of learning capacity appeared. In the initial learning, the domestic animals were best; in tests for simple transfer of training, the aments; and in training for reversal of the habit, the chimpanzees. The authors, emphasizing the possibility of various defects of their method that seem (to this reviewer) inherently unavoidable, have provided a clear picture of the comparative problem when one is trying to study such a variable as rate of learning, for example, with motivation controlled (or emotionality with intelligence constant). One aim of the authors seems to be to leave no doubt of the need for better conceptions for the comparative study of behavior.

Other studies are comparative by implication. Delayed response was originally considered to be an index of ideational or symbolic processes, a "higher" form of behavior. It is this context, it seems, that makes it significant to demonstrate something like delayed response in the minnow (7) or, with a maintained bodily orientation during locomotion, in the octopus (8). The study of "instinctive behavior" is also essentially comparative, psychological interest in (*a*) the species-predictable (or instinctive) pattern of response deriving from the contrast with (*b*) "learned behavior" which varies according to the concatenations of previous sensory stimulation to which the individual animal has been exposed. A review of some of the work in this field by Lorenz and his pupils is to be found in a paper by Tinbergen (9). More interest continues to be maintained in the behavior of nonmammalian species on the European continent than in America, and the point of view maintained is on the whole distinct from the American one, while still quite as objective. Those interested should also consult the recently established journal *Behavior* (10). The existence of independent points of view has, of course, considerable value for psychology, opening new avenues to behavioral experiment

and providing a corrective against concentration on the study of too few species or narrowness of theory.

## Behavior After Damage to the Brain

In turning now to various aspects of physiological psychology, the reader is first commended to Harlow's general review (11) of the field for 1948 and to Bender & Teuber's review (12) of the "psychopathology of vision" in which emphasis on Goldstein's approach (66) to neurological problems is combined with a critique of current ideas about agnosia, alexia, and so on.

A very interesting and important paper by Bard & Mountcastle (13) has provided new information about the mechanisms of emotional behavior. The removal of neocortex in the cat does not produce sham rage but placidity, if some of the cingulate gyrus and pyriform cortex are spared, with amygdala and hippocampus also spared. Remove either the rest of the cingulate or remove pyriform lobe, amygdala, and hippocampus and ferocity replaces placidity. (Some of the results reported are, as the authors point out, inconsistent with observations reported by others, and no explanation of the inconsistency is at present available.) It is of interest to note that the suppressor function of the cingulate gyrus, to which one might attribute the extreme placidity, was still present in those cats in which ferocity was produced by removal of pyriform cortex, amygdala, and hippocampus. It is equally important to note that Ward (14, 15) has described the effect of removal of the cingulate suppressor area (Brodmann's area 24) as producing a condition of loss of fear, and changed social perceptiveness, but not sham rage, nor hyperactivity. The doubts involved in interpreting the function of suppressor areas as the equivalent of a higher inhibitory process will be returned to farther on. Here it may be observed that we appear to be on the threshold of a new and real understanding of subcortical forebrain function, in which the anatomist is coming into his own with an essential part to play instead of just providing illustrations for textbooks in psychology or neurology.

The studies of brain-injured soldiers by Teuber & Bender (16, 17) and Wortis, Bender & Teuber (18) are important sources of new data on the psychology of perception. The data are not easily assimilated into existing theory but presumably are the more important for that. These clinical studies usually lack good anatomical data, and it is not known whether the defects described are due to hypofunction or to dysfunction, but theory must eventually comprise them. The phenomena reported include reduction of flicker-fusion rate in the peripheral field with no change of rate in the macular field. Changes in the perception of apparent motion are found in the same parts of the visual field as reduced flicker-fusion rate. There may also be "extinction": an object which can be seen by itself vanishes as soon as a second object is introduced into another part of the field. Changes in pattern vision include alteration of orientation,

micropsia or macropsia, deformation of contours, and "monocular diplopia" or polyopia. Tachistoscopically, a presentation time of 0.1 sec. minimizes the defects in pattern vision; with shorter exposure contour formation is incomplete, and with longer exposure distortions increase, with the 'drift" to a wrong location sometimes being directly observed by the patient. The authors (17, 18) consider that the effects may be due to lesions outside rather than inside area 17, and they note the obvious relationship of some of their results to the figural aftereffects of Köhler & Wallach (64).

Two experimental studies of vision after extirpation of the occipital-lobe tissue bordering on area 17 have been made, with dissimilar results, by Ades & Raab (19) and Lashley (20). Ades & Raab, essentially extending an earlier study by Ades, removed "areas 18 and 19" on both sides simultaneously in one monkey and observed loss of an experimentally established discrimination (but not a general agnosia). Removing one side first, retraining, and then removing the second side did not result in loss of the habit in a second monkey. Removal of the temporal-lobe cortex in the same animal then produced a lasting deficit. In two other monkeys, bilateral removal of temporal cortex alone produced no defect; but subsequent destruction of occipital association cortex again abolished the habit. The authors note that their results, after bilateral temporal decortication, are not in agreement with the well-known results of Klüver & Bucy (65) and suggest that the differences may have resulted because in the present study the hippocampal region and the optic radiations were left intact.

The study by Lashley (20) was specifically a search for an experimentally produced agnosia in monkeys. Approximately the same preoccipital area was removed as in the study by Ades & Raab, though no temporal-lobe destruction was made by Lashley. In some of his animals, frontal-lobe destruction was added to the occipital-lobe destruction. Considering the preoccipital operation alone, the defects obtained by Lashley were less clear-cut than those of Ades & Raab; and the basis for this discrepancy is not clear at present. In reporting his results, Lashley has emphasized the lack of defects in his monkeys, although he did find loss in delayed reactions, in the four of his five animals which had larger occipital lesions, and defects in the "conditional reaction" as well. Bilateral removal in the preoccipital area produced no defect of form-discrimination habits. The conclusion is drawn that agnosia in man is not the result of a simple destruction of transcortical association fibers, and this is supported by rather convincing reference to clinical evidence which indicates that agnosia is a product of dysfunction rather than lack of function of some particular structure. The question is not raised as to how excitations normally depart from area 17, if it is not via 18 and 19; the alternatives seem to be either (*a*) that there are efferent fibers from area 17 to the geniculate and neighboring thalamic structures or (*b*) the approximately

40 per cent of areas 18 and 19 which was left undamaged is sufficient to mediate ordinary visual discriminations. This study (20) is of value in its discussion of methods for the efficient examination of vision in animals.

Negative results are also emphasized by Blum (21) in a most important study of two chimpanzees with prefrontal association cortex removed. No defect of delayed response was observed, even under conditions of considerable distraction; and the emotionality of the animals, their capacity for frustration responses, was very marked following operation. The report, of course, is inconsistent with Jacobsen's celebrated study of Becky and Lucy. This earlier study, a preliminary report of which was given by Jacobsen & Fulton at the second International Congress in Neurology in 1935, is presented again in detail by Crawford, Fulton, Jacobsen & Wolfe (22), presumably because of its historical significance (it is said to have suggested the lobotomy procedure to Moniz). Contrary to the impression given by secondary reports of this early study of frontal cortex in the chimpanzee, no complete loss of delayed reponse is actually described by the authors in the present report. In most respects, the animals' behavior was eventually very nearly at a preoperative level on "good" days, although it also vacillated since they had many "bad" days. The vacillation was evident even after a recovery period of two and one-half years. At this time there were no simple, clear-cut defects to be observed. In short, "the outstanding feature of the adaptations following frontal ablation is the erratic and variable character of performance" (22). This inevitably raises the question of the presence of an epileptoid dysfunction, as an explanation of the defects when they were present. It is known clinically that epileptoid disturbance may occur without convulsions, or even without a recognizable petit mal type of arrest of activity. If this was the source of the defects observed in Becky and Lucy, as a by-product of scar formation, the discrepancy between Blum's results (21) and those of Jacobsen (22) can be understood provided there was for some reason, less scar formation in Blum's animals.

The possibility of such an explanation is greatly strengthened by the report of Blum, Blum & Chow (23), who "uncovered" a focus of Jacksonian epilepsy by the administration of amphetamine to a monkey in which bilateral frontal-lobe removal had been done earlier, with no unusual behavioral signs until the drug was administered. This appears to be a possible clue also to the nature of the characteristic hyperactivity of monkeys after frontal-lobe operation, in view of the temporal course of its development after the tissue has been removed.

Harlow & Settlage (24), using monkeys, have compared the effect of bilateral frontal extirpations with unilateral extirpation on a series of learning problems: discrimination learning (15 problems), discrimination reversal (10 problems), and several other tests. The animals with bilateral prefrontal lobectomy were inferior to normal animals on all tests. The defects were at least as great as those due to hemidecortication, although

the hemidecortication meant a greater total destruction of cortex. Hemidecortication also produced defects in all tests. This study and that of Bromiley (25) are noteworthy as among the very few in which intellectual function has been studied in animals after unilateral destruction. Bromiley has shown a significantly slower rate of conditioning in unilaterally hemidecorticated cats. These studies definitely deny the commonly made suggestion that a second hemicortex is a "spare tire" except for its motor and sensory functions.

Harlow & Settlage (24) agree with Blum (21) in finding that removal of the frontal lobes did not reduce frustration responses, which were more marked for the frontal-lobe monkeys than for hemidecorticates and normals. Also, the frontal-lobe animals were harder and the hemidecorticate animals easier to tame than the normals.

In the clinical field, the frontal lobe and the frequency with which lobotomized patients are available for study continue to tempt the unwary student of cerebral localization to draw hasty conclusions, positive or negative. It is reasonably certain that damage to man's frontal lobe inevitably produces intellectual and emotional changes. But it is a long step, from the kind of evidence available, to a demonstration that some particular function is "localized" in the frontal lobes. Before one can establish this, it is necessary first to show that the patient after frontal lobe operation has a lower level of understanding, or a changed emotionality, as compared with his condition before disorder of any kind had occurred; and secondly, that the change is due simply to a loss of tissue, and not to an active neural disturbance of some kind. To establish both of these points has not yet been achieved, and we have as yet no clear and definite evidence concerning the function of the prefrontal association areas in man.

But, on the present evidence, it is nearly as difficult to show that they do not have some function. It is still unjustified to conclude that intelligence is unaffected by frontal lobe damage. Pre- and postoperative comparisons in frontal lobotomy procedures are apt to show little apparent effect of the operation. But a preoperative examination, in a patient whose behavior and emotions are sufficiently deranged to justify lobotomy, is most unreliable as an index of the premorbid level of intelligence. In fact, in pre- and postoperative comparisons in lobotomy cases, a rise of test scores is not infrequently observed. This should raise suspicion of the preoperative rating, no matter how good the rapport in the testing procedure may have seemed to be; and now a report by Koskoff, Dennis, Lazovik & Wheeler (26) supports the suspicion. In their series of five cases (in which both pre- and postoperative Wechsler scores were available, in a total series of 10 cases), lobotomy was done not because of mental illness but for the relief of intractable pain. A mean drop in IQ of 20.4 points was observed. A markedly similar result is reported in one case by Yacorzynski, Boshes & Davis (27); although the patient was

neurotic or psychotic, exceptional co-operation was obtained, and the Stanford-Binet IQ dropped from 118 to 97. Again, Rosvold (28) was able to obtain premorbid measures of intelligence, made in the Canadian Army, for seven veterans who were later hospitalized with psychosis. The pre- and postoperative Wechsler scores showed little or no change in intelligence after lobotomy; but data were available for transforming Army classification-test scores into Wechsler scores, and the estimated original IQ's were found to have dropped by 19 points. There can be little doubt that lobotomy in a normal brain would induce serious defects of problem solving, and equally little doubt that the operation does not restore the original level of intellectual function in the psychotic patient.

A number of studies have shown that ECS (electroconvulsive shock) in rats produces definite behavioral changes. Duncan (29) tried to show that ECS eliminates recent learning selectively. He did get disturbance of learning but it may not have been selective, since the performance of his experimental group is not significantly different from chance. Thus, it may have meant only a state of confusion. A second experiment (30) led to the conclusion that new learning in the rat requires a period of consolidation, running up to 15 min. or so; during this period ECS interferes with retention more than it does later. Hayes (31) shocked animals daily in the training period, with a decrement of performance which is attributed to "confusion"; it is stated that brain damage due to the passage of current did not cause the confusion, but unless the author is referring only to gross damage it is not clear how this conclusion is established; changes of permeability in the cell, or analogous damage, can hardly be excluded. Stone, Eriksen & Porter (32, 33) have found that defects, due to shocking the infant rat (20 to 29 days), which are demonstrable 10 days later are still evident at 74 to 90 days. Braun, Russell & Patton (34) similarly showed that defects induced in adult rats are as evident 60 days after the shock series as immediately after it.

Maternal behavior and pregnancy have been found by Rosvold (35, 36) to be seriously affected by ECS. The changes are more extensive and clear-cut than reported changes in maze learning. The author concludes that the effects are greater than can be accounted for by the probable degree of damage to the cerebral cortex, and some of the effects appear to involve endocrine disturbances (there was evidence of damage to the pituitary).

The effects on pregnancy, parturition, and maternal care depend essentially on the time at which shock is administered. ECS within 15 hours of insemination, for example, led to resorption of the implanted embryos; but pregnancy was maintained in from 40 to 60 per cent of animals when ECS was given 36 to 48 hours after insemination. Shock in the latter half of pregnancy prolonged parturition and made labor difficult. If shock was continued up to the first day postpartum it obliterated

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maternal behavior; after this period, the effect on maternal behavior was less complete.

### Stimulation Methods

Two important studies of speech mechanisms by Robb (37) and Penfield & Rasmussen (38) have exceptional importance in their use of the stimulation method and so are discussed together in this section, although Robb's report includes results obtained by extirpation methods as well.

The first part of Robb's work (37) describes mainly a series of surgical removals in or near the speech area, in patients operated on for removal of epileptogenic foci. No very clear picture of the tissue that must be left intact to permit speech emerges, although a number of diagrams are presented which show individual surgical removals that did not produce chronic aphasia. A significant part of the data concerns the incidence of a delayed aphasia, in which the slow recovery cannot be ascribed to vicarious functioning since no aphasia was present immediately after operation but did develop within a day or so with postoperative local edema of the brain. Such data ultimately promise either to localize the speech area exactly, or to show that it cannot be localized.

But a most valuable development is the use of the stimulation method, under local anesthesia, in outlining and analyzing the functions of the speech area (37). In 33 craniotomies on the presumptively dominant side, various disturbances of speech, due to stimulation which temporarily "paralyzed" a small area of cortex, were observed: (*a*) complete arrest of speech; (*b*) a slowing of speech; (*c*) production of jargon; (*d*) ability to use words but not to name objects. In the nondominant side, only the first two effects appeared, and these only with stimulation in the primary motor and sensory areas, close to the foot of the central fissure. It appears that a simple arrest or slowing of speech may be, so to speak, peripheral, or motor, i.e., a disturbance of speech production, not of the central mechanisms of speech. This was observed on both dominant and nondominant sides. But on the dominant side, arrest and slowing were observed well outside the motor area; this effect presumably is central rather than motor. Penfield & Rasmussen (38) have shown that the one type of speech arrest, in the "Rolandic zone," is elicited from the same motor or sensory area as that from which a primitive vocalization or a movement of tongue and lips may also be elicited. They consider that arrest of speech here is due to an interference with articulation, while "frontal speech arrest" (from an area approximating to Broca's area) and "parietal speech arrest" (centering above the posterior Sylvian fissure) are due to interference with the central processes of thought that antecede speech (38). Consonant with this, Robb (37) reports that jargon and loss of names with retention of other speech were produced almost without exception from the parietal and nearby temporal areas.

These results offer a new basis of attack on the organization of speech

and are clearly most important as such; but they also have another significance, in drawing attention to a common error of interpretation when analyzing neural function by means of stimulation experiments. The point to which these data draw attention is that electrical or strychnine stimulation of a neural mass may produce effects precisely opposed to the normal function of that mass. Stimulation that is definitely in the speech area does not increase speech, but disrupts or abolishes it. How, or why? To speak of the stimulation as "paralyzing" the speech centers may be misleading in this context, for it may be taken as a too easy verbal solution of the problem. What is the mechanism of paralysis?

Modern neurophysiology has been making it plain for more than 10 years that the functioning of synapses depends on a precise timing of activities in three or more cells (two afferent and one efferent) and the obvious inference is that the functions of a complex neural system must depend equally on time factors. It may be considered that there are two types of transmission systems in the brain and spinal cord: (*a*) systems such as the optic nerve and optic tract, the pyramidal tract, or any one in which all cells stimulated within a given small region deliver impulses together to a second small region, so that their effects can sum and take control of efferent cells leading from the second region and (*b*) systems in which neighboring cells conduct to widely scattered points and so do not have this summative action. Stimulating a point in the first type of system will have effects similar to its normal physiological action; but stimulation of contiguous cells in the second type of system may simply fire, all at once, cells which for their normal function must fire separately. Specifically speaking, sensory systems, such as the optic or the somesthetic tract, are designed to pick up and transmit punctate stimulations to one point in the cortex; but the speech areas and other association areas of the cerebral cortex may be designed instead to handle a complex series of excitations diffusely scattered through the tissue. When the tissue is functioning normally, there is no evidence of increased local potentials, as there would be if the system operated to concentrate excitation at a point or series of points. A gross stimulation, consequently, must prevent the system from having its normal pattern of diffuse firing.

Tactual and visual systems thus can be regarded as designed for a type of transmission in which a minimal stimulation has a magnified effect at a single point, an optimal arrangement for achieving some sensory control of the internuncial cerebral systems in which transmission is quite different. Pain might then be regarded as the result of tactual excitation without sufficient control of its central effects. Weddel, Sinclair & Feindel (39) have proposed that hyperalgesia, after peripheral nerve injury, is elicited only from points at which there is a reduction in the number of contiguous afferents stimulated, that is, when sensory control of cerebral processes is diminished. This would suggest a reason for the frequently observed hyperalgesia following central nervous lesions and for

the relationship of "normal" pain to the relatively inefficient C fibers (which might be regarded as quite capable of setting up a central excitation but as less capable of maintaining organization in it) and yet not make it necessary to suppose either that C fibers alone arouse pain, or that they always must do so.

The pyramidal tract, similarly, seems designed to deliver excitations from one part of the cortex to one limited set of cells in the stem or cord. Consequently, punctate stimulation in an experiment may produce effects that bear good relation to the ordinary functioning of the system, unlike stimulation of the speech areas.

But there is a question whether it is ventral horn cells, or some internuncial system, to which the pyramidal tract delivers its more or less concentrated excitations. The "muscle-mosaic" theory of motor cortex (area 4) organization implies the first alternative, a direct control of ventral horn cells. Clark (40) has reviewed this and the theory of "representation of movements" in area 4, with emphasis on the complexity of the evidence (which does not fit either of these historically opposed points of view very well). He suggests that the stimulation of a point in the motor cortex is delivered to a "functional group of spinal internuncial cells" which would also be influenced by other sources of excitation. With any one background pattern of excitation, stimulation of a point in area 4 would then have a constant peripheral effect, but events elsewhere would be capable of modifying this effect. Thus Clark & Ward are reported to have found reversals in response with reversal in the initial position of the limb (40). Gellhorn (41) has reported changes in motor response with variations of posture, but did not, however, observe any reversals.

Delgado and Livingston (42) have found closely related phenomena with stimulation of the orbital surface of the frontal lobe: changes in frequency and intensity of stimulation of a single cortical point may produce very different peripheral effects. The same point may be "inhibitory" or "excitatory."

Denny-Brown & Botterell (43) have also shown that the action of segments of area 4 is less specific than one usually assumes: "The paralysis which results from ablation is greatest and persists longest when the whole of area 4 is extirpated. Survival of any quadrant lessens the intensity of the resulting disorder in all the contralateral parts."

### Suppressor Functions and Thought

The question of the way in which a system will respond to punctate stimulation, of whether the result will mirror the ordinary functions of the system, can be applied to the suppressor systems of the cortex and brain stem. Psychologists must be wary of assuming that, because stimulation of the suppressor areas in anesthetized animals has an inhibitory end effect, the function of such areas in the normal waking animal is to prevent activity in other areas (44, 45). As Magoun has pointed out (46),

it is known that the activation pattern throughout the cortex (including suppressor areas), when an animal is aroused to wakefulness, is typically a reduction of amplitude in cortical potentials, which implies that cortical function within as well as outside the suppressor areas depends on diffuse patterns of firing. Stimulating simultaneously all the cells within one small region of a suppressor area, therefore, may negate the normal function of that area as it does in a speech area. Clark (47) has reviewed the evidence of suppressor function, quoting McCulloch as saying that suppression takes place by an interruption of the usual rate of corticothalamic reverberation. Such an interruption need not be produced by a primary inhibition. It should be observed also that the first effect of stimulating a suppressor area is a loss of potentials within that area itself, followed by loss elsewhere; and nearby areas may be recovering while more remote areas are still depressed. These facts are not consistent with a primary inhibitory action of the suppressor area itself.

Furthermore, Clark & Ward (48) made observations on the effect of stimulating both suppressor and motor areas in unanesthetized monkeys, with implanted electrodes. Relaxation was observed, but not with the frequency that would be expected on the basis of the experiments done with anesthetized animals, or with evidence of any general inhibitory effect. Stronger stimulation of the same points gave motor movement. Also, prior stimulation of a suppressor area "potentiated" response to stimulation of a motor point (47).

Ward, McCulloch & Magoun (49), studying the motor systems of the brain stem, were able to produce a state of "paralysis agitans" by lesions well below the basal ganglia, destroying part of the mesencephalic and pontile tegmentum. The tremor rate was not related to cortical activity rates, and the authors conclude that it is produced by rhythmical activity of the reticular formation of the brain stem. Other studies by Lindsley, Schreiner & Magoun (50, 51) have produced spasticity by lesions of both cerebral and cerebellar suppressor systems, the effect lasting longest and being most permanent when both cerebral and cerebellar systems were damaged. The spasticity could then be abolished by transection of the pontobulbar tegmentum plus destruction of vestibular nuclei. These studies have shown the existence of both a suppressor and a facilitatory system in the brain stem, so far as efferent effects are concerned; but a further phenomenon has been found by Magoun & Moruzzi (46) (just to keep the picture from being too simple). Stimulation of the "suppressor" system of the bulbar reticular formation produces the cortical activation pattern in lightly anesthetized cats. The effect is not due to stimulation of long pathways, but to multisynaptic reticular pathways ascending into the diencephalon. Evidence is also reviewed to indicate that this system must be involved normally in the arousal reaction.

It may be pointed out that existence of the "waking center," which has been known for some time, may well be one part of this system, and

Magoun (46) has attributed to the system exactly the function that Morgan (67) referred to as a "priming" of the cerebral cortex by the waking center.

With the foregoing experiments, and the important studies of Jasper and his associates (52), we are getting closer to the possibility of a physiological analysis of cerebral organization in the waking state, which implies an analysis of thought. Mention should be made here of the very interesting report by Kennedy, Gottsdanker, Armington & Gray (53) of "kappa" waves during intense thought, although it may be early as yet to try to evaluate the results. The discussion by Wiener (54) of the parallels between computing and directing machinery and the human brain is also beyond the power of this reviewer to evaluate fully. It provides a new formulation of old problems and is likely to have considerable influence on neurological ideas, in the parallel between (*a*) certain defects in the machine and (*b*) pathological conditions such as intention tremor, in that collection of cybernetic devices, the nervous system. It appears that neurologists, like psychologists, will have to review their mathematics.

### Psychosomatics, Stress, Anxiety, and Schizophrenia

To this reviewer one of the most significant and hopeful developments in recent psychological work is the increased attention to the physiological mechanisms of symptom formation in such psychosomatic studies as those of Wolff and his co-workers (55 to 58) or in objective analyses of stress and anxiety such as Malmo & Shagass (59, 60) have initiated, and particularly in the work of Hoagland, Pincus, *et al.* (61, 62, 63) on adrenal cortical function in normal persons, neurotics, and schizophrenics. No attempt is made here to give a complete picture of the work, but only to cite a few papers which may be representative of a field with which the reviewer has only a superficial acquaintance, in the conviction that the developments are too important for physiological psychology to be left unmentioned.

A long step toward an understanding of schizophrenia appears to have been made (61). Whether one is familiar with the formidable symbols and terminology of biochemistry or not, it appears that if one is seriously interested in the problems of mental illness some understanding of the work on adrenal cortical hormones is essential. The schizophrenic shows significant deviation from the normal in cortical hormone output, both in ordinary circumstances and under stress. "Stress" includes: the stress of getting up in the morning, severe exercise, low oxygen supply, extreme cold or heat, surgical operation, injection of toxins, or swallowing large amounts of sugar. In normal persons, such stresses increase adrenal cortical hormone output; in the schizophrenic patients, as a group, they fail to have as much effect. The ordinary schizophrenic is not like a patient with Addison's disease, since he definitely has a functioning adrenal cortex, but apparently approaches this condition. It is not suggested (61)

that defects in adrenal cortical function are the whole story of schizophrenia, but there clearly is a physiological as well as a psychological derangement in a majority of schizophrenic patients, and even though psychological factors may have an essential part in bringing about the derangement it seems likely that the resulting physiological condition will have to be understood and remedied before psychotherapy can have much value for the patient. It has long been known that psychotherapeutic methods have little efficacy in psychosis; if we can discover how to restore normal endocrine function first, it may become possible for psychological weapons to finish the job.

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# ABNORMALITIES OF BEHAVIOR[1]

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Reviews of psychiatric literature for 1947 and 1948 include brief references to empirical and experimental research, and to clinical investigations into behavior pathology, particularly in the sections on clinical psychiatry (1 to 4), child psychiatry (5 to 8), neuroses (9, 10), alcoholism (11 to 14), psychoanalysis (15, 16), geriatrics (13, 14), and psychosomatic medicine (3, 4, 17, 18). Bellak (19) provides a comprehensive review of a decade's literature on schizophrenia, 1936-46, thus continuing the documentation begun 10 years earlier by Lewis (20). Bellak's review includes a section on psychological factors in etiology, pathogenesis, and pathology, a section on psychological diagnostic criteria, and chapters on psychological and physiological factors. The bibliographies in these areas are far from complete.

In a ten-year review of the literature on headache, Friedman (21) indicates the increasing emphasis which is being placed upon the effects of anxiety, fear, and depression. Coleman & McCalley (22) survey the literature on nail-biting, 1927-47, and include a number of studies of this behavior among military personnel of World War II. Bruch (23) summarizes recent findings and interpretations of obesity as a consequence of maladaptive personality maturation.

Karpman (24) furnishes a review of psychoanalytic contributions to the problem of coprophilia, based on the writings of Freud, Ferenczi, Abraham, Kempf, Jones, and Roheim. Eighteen papers by Klein, on the psychoanalytic problems of childhood, 1921-45, have been brought together in a single volume (25), and a collection of essays and clinical studies by Isaacs has appeared (26). To these primary contributions to psychoanalysis should be added notice of a published bibliography of 65 publications by Freud in the first 20 years of his productivity (27), and an article by Johnson (28) which attempts to evaluate psychoanalytic procedures as well as concepts.

In the first of two volumes, which together will constitute a revision of his earlier *Medical Value of Psychoanalysis*, Alexander presents his view of contemporary psychoanalysis (29). In line with current trends, to which Alexander and his associates have given considerable impetus, this presentation emphasizes cultural influences on personality development, psychosomatic interpretations of sexuality, and flexibility as a guiding principle in the application of therapy. Menninger, in a carefully documented book (30), summarizes the psychiatric achievements in World War II and describes with candor the special problems that arise when

[1] This review covers the period from January, 1948 to May, 1949.

there is a lag in numbers, training, and attitudes of medical personnel relative to existing psychiatric needs in military service. To some extent the volume is a text in psychiatry, since Menninger analyzes the pressures and personality patterns which lead, in a military setting, to behavior disorder. To a lesser but still important extent, it contributes, in its emphasis upon esprit de corps, leadership, and morale-building techniques, to the understanding of peacetime community problems and of preventive psychiatry.

Cameron (31) attempts a systematic formulation of behavior disorders from a biosocial point of view, which begins with the normal or abnormal person as a biological organism operating in, and by means of, a social environment. Considerable emphasis is placed throughout upon communication, learning, role-taking, and socially derived self-reactions. Burrow (32) gives a phylobiological analysis of neurotic developments in man. An appendix summarizes some of the Lifwynn Foundation instrumental research in relation to cotention and ditention. There is a useful glossary of phylobiological terms and concepts.

With the publication of *Compulsion and Doubt* (33), the task of rendering into English the encyclopedic work of Stekel, *Disorders of the Instincts and Emotions*, has been completed. A prefatory essay by the translator, Gutheil, provides the reader with a well-integrated introduction to Stekel's approach to obsessive and compulsive phenomena. Lief (34) has edited 52 selected papers by Adolf Meyer, and cemented them together with biographical narrative that relates their content to Meyer's active professional life.

## Etiology

The question of etiology of behavior disorder continues to stimulate a variety of approaches. The concept of psychogenesis is subjected to strict logical analysis by the philosopher Reid (35), who concludes that by a psychogenic disorder is meant one which includes among its antecedent events some acts of emotionally reinforced interpretation.

Research studies on heredity and eugenics during 1947, as well as new books and journals, are reviewed by Kallmann (36) in an article which notes a number of foreign papers not easily accessible. Published research for the year is largely on mental deficiency, sensory defect, and animal experimentation. The reviewer also mentions studies on psychopathic personality and alcoholism, and his own work on the inheritance of schizophrenia. The latter is summarized in Kallmann's contribution (37) to Hoch's volume (138) on treatment. A recent critical review of Kallmann's *Genetics of Schizophrenia* (139), prepared by Pastore (38), calls into serious question the evidence for the genetic determination of schizophrenic disorder. Pastore analyzes with care the diagnosis and selection of Kallmann's sample, his expected frequencies of schizophrenia, and his failure to control certain environmental variables. Pastore concludes that the genetics of schizophrenia is still an open question. A brief note by Gamble (39) presents figures on the total number of psychotic patients sterilized under state law up to January 1, 1947.

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An extension of Sheldon's somatotyping (140) to cases of behavior disorder is reported in an article by Wittman, Sheldon & Katz on 167 state hospital patients (40). Bellak & Holt (41) have investigated the distribution of somatotypes in a group of 50 schizophrenic patients and a group of 33 paretics, all of whom had died in hospital and had been photographed at necropsy. They found the distribution similar in the two groups, and, in both groups, considerably different from Sheldon's figures on college men.

Klein & Horwitz (42) studied the expression and handling of sex drives in 80 paranoid and paranoid schizophrenic patients, 40 of each sex. They concluded that, however pertinent in some cases, homosexual conflict could not alone account for the paranoid mechanism. They found, for most of their patients, distrust-provoking backgrounds, preoccupation with competitive striving, crucial precipitating situations, and guilty attitudes toward sexuality, especially masturbation. The majority picked a male figure as first hostile figure and as chief persecutor. This study drew a letter of objection (43), and then a reply (44). Using a rather widely inclusive set of criteria for its presence, Norman (45) reports homosexuality in 55 per cent of paranoid schizophrenia, in 28 per cent of catatonic schizophrenia, and in 44 per cent of anxiety psychoneurosis (chronic alcoholism); all were hospitalized.

Wittman (46) attempts to distinguish between constitutional or "process" schizophrenia, with insidious onset and a history of psychological referral in childhood, and "schizophreniform" disorders, with relatively acute onset and no history of childhood referral. On theoretical grounds, Ausubel (47) proposes a classification of schizophrenic disorder into two groups, evolutionary and reactive.

On the environmentalist side of the question, Henry (48) reviews his observations on Pilaga Indian child-rearing and states that abnormal symptoms develop only if the cultural environment provides the necessary conditions. In contrast with the permissive attitude of the Pilaga toward elimination, feeding, and sexuality, the prohibitive American ones are more likely both to define and to intensify anxiety symptoms.

In a study aimed at determining the incidence of neurosis in different occupational groups, Smith & Hightower (49) obtained the following results. In a control series of 300 consecutive admissions to the Mayo Clinic, 22.7 per cent are reported as showing purely functional disease, and 7 per cent as mixed functional and organic. For the occupational groups, the following percentages of purely functional plus mixed functional and organic are given: high school and grade school teachers, 54 per cent; nuns, 42 per cent; clergymen, 39 per cent; lawyers and housewives, 36 per cent; dentists, 30 per cent; farmers, 19 per cent; physicians, 17 per cent; railroad engineers, 9 per cent; unmarried women, used as a control group for the nuns, 32 per cent; Jews, studied as a group because of high incidence usually assigned to them, 30 per cent. The population is representative only of persons seeking help in a medical clinic. Clark (50) analyzed the histories of 332 male schizophrenic first admissions in the Chicago area

and found significant negative correlations between occupational schizophrenic rates and both income and occupational prestige.

In a study of heredity, personality pattern, onset and dynamics in 64 cases of simple schizophrenia, Kant (51) concludes that the simple subtype develops from prepsychotic nonassertiveness in a disorganized early home environment. Tietze (52) presents in narrative form the data she obtained from interviewing 25 mothers of schizophrenic patients. These data include appearance and behavior in the interview situation, relationship with the interviewer, reported sexual orientation and marital adjustment, parent-child attitudes, and reaction to the occurrence of the schizophrenic illness. In a comparison of 63 clinically allergic children with a control group of 37 nonallergic ones, Miller & Baruch (53) find a significant preponderance of rejecting mothers in the allergic group.

Psychopathic personality remains a controversial topic, with adherents of the hereditary, constitutional, and environmental views all represented in the literature. Ripley & Wolf (54) indicate that the restriction provided by an island combat setting may lead to psychotic episodes in psychopathic soldiers who have an early history of enuresis, temper tantrums, nightmares, and sleep-walking. Cason (55) reports an attitude scale which differentiates psychopathic from other federal prisoners. Gough (56) applies the concept of role-playing to the behavior of patients diagnosed psychopathic. Inverse relationships between neurotic and psychopathic patients are reported in studies by Rodgers (57) and Michaels & Porter (58). Familial similarities in bodily complaints (59) and in EEG pattern (60) among psychopathic patients are used to support the hereditary hypothesis. New classifications of psychopathic personality are advanced by Cason (61) and Tarjan (62), while Karpman (63, 64) favors the deletion of the category altogether and the substitution of the concept of anethopathy. Arieff & Rotman (65), on the other hand, urge further exploration of the constitutional basis of this disorder.

## Learning

Two papers continue the current trend toward assimilating contemporary learning theory to behavior pathology. In an analysis of the "neurotic paradox," Mowrer (66) reviews the steps taken by Freud in attempting to explain why neurotic behavior still persists when it seems to have unfavorable consequences. This riddle, left unsolved by Freud, is unravelled by Mowrer's assumption that there are two distinctly different learning processes—conditioning and problem-solving. Secondary drives such as fear, acquired through conditioning, may be reduced in the course of problem-solving; and their reduction reinforces the learning of reactions which appear to be perseverative or nonrewarding to the organism. According to this view, neurosis reflects a learning deficit: the neurotic adult, through problem-solving, has learned to keep from being emotionally and attitudinally conditioned.

The second paper, by Misbach (67), emphasizes Freud's definition of the ego as "organization" and seeks to fit this definition into the frame-

work of the centralist learning theorist's "perceptual closure." Anxiety, and the circumstances which Freud believed gave rise to it, are seen by Misbach as threats to organizational integrity. In the same vein, the writer believes that excessive gratification and frustration, often invoked as antecedent conditions to neurosis, are significant only insofar as they restrict the growing child's field of operations and interfere with the differentiation and expansion of his ego.

McGill & Welch (68) analyze the development of hysteria as a case of Pavlovian conditioning and review relevant clinical and experimental findings to support their hypothesis. A specific analysis of therapy in terms of peripheralist learning theory is provided by Shoben (69). Smith's brief account of bed-wetting (70) interprets enuresis as the product of inappropriate learning of cues to micturition, in a family setting which favors training in bed-wetting, rather than training in urinary control. The attitudes, needs, and anxieties of parents receive consideration as etiological factors, although the act of bed-wetting and its retraining are analyzed in terms of associative learning. Although the volume is directed toward parental retraining, its last section brings together a number of experimental studies on factors affecting enuresis.

Two papers by Huston & Shakow (71, 72) present results of experiments in pursuit learning in 122 schizophrenic, 13 depressed, and 60 control subjects. The schizophrenic patients showed significantly poorer initial and trial-to-trial achievement on this task than either the depressed or the normal subjects, an inferiority which seemed to be related partly to poor co-operation and partly to the clinical subtype of schizophrenia in which the patient had been classified. Differences between schizophrenic and normal performance virtually disappeared after 33 days of practice; but only nine patients and two controls contributed to this result. These findings are taken as evidence that deterioration of learning capacity in schizophrenic patients is not irreversible.

### Induced Behavior Pathology

Inevitably intertwined with the extension of learning theory to behavior disorder are laboratory studies of induced pathological reactions. In a paper reporting three separate studies, Miller (73) demonstrated object-displacement and drive-displacement in white rats. Animals trained to strike one another attacked a celluloid doll when they were alone in the cage. Thirsty rats trained to run down an alley to water ran faster when they were hungry and satiated with water than did control animals. As compared with their controls, hungry rats, trained to run a maze to food, ran faster and more accurately when they were satiated with food but received electric shock. In interpreting his findings by way of stimulus- and response-generalization, Miller makes a convincing case for the application of conditioned response principles to the psychoanalytic phenomenon of displacement.

From a somewhat different theoretical orientation, Marquart (74) studied the effect of punishment upon fixation in the behavior of college

students. She employed discrimination learning problems and no-solution problems, of the sort used by Maier (75) and his associates, in studying abnormal behavior in rats. Although it is doubtful that more that one of her subjects showed real fixation, Marquart's findings justify the conclusion that concentrated punishment is more frustrating than less concentrated punishment. Furthermore, the bimodality of some of her distributions provides support for Maier's hypothesis that frustration and motivation are qualitatively different processes (75).

In this connection should be mentioned the critical analysis by Glixman (76) of three experimental studies of repression published by Rosenzweig (141), Alper (142), and Glixman. These studies, utilizing an interruption technique under varying conditions of stress, yield divergent results and contradictory interpretations. Glixman criticizes the confounding of variables in the first two experiments, particularly that which results from combining recall scores from incompleted and completed tasks. His own reanalysis of Rosenzweig's and Alper's data, when these variables are separated, provides results which support conclusions different from those stated by these two investigators. Even with this methodological ambiguity removed, the three experiments taken together are still contradictory. The implications of this analysis for the design of future studies dealing with repression are probably more significant than the experimental results obtained.

That experimental semistarvation in adult males may eventuate in personality alterations is indicated by the report of Schiele & Brozek (77) on the behavior of 36 volunteer conscientious objectors who participated in the University of Minnesota starvation-rehabilitation experiment. Average Minnesota Multiphasic Personality Inventory profiles for the group typically showed an elevation in neurotic scores during the semistarvation period. Nine subjects developed such pathological symptoms as hysteroid sensory and motor reactions, psychogenic accidents, and mild disorganization or disintegration. The authors state that the personality disturbances, since they developed under the stress of semistarvation and receded during controlled rehabilitation, can be considered an experimental neurosis. They emphasize, in addition, the particular background factors which may have contributed to differences in individual susceptibility.

In the course of hastening hypnoanalytic treatment of a male veteran of World War II who had developed right hysterical hemiplegia, Leavitt produced hypnotically a secondary and tertiary personality in the patient (78). Instructions under hypnosis, that these personalities should reflect the "good" and "bad" sides of the patient, resulted in two separable roles, which Leavitt equates with the superego and the id, in contradistinction to the ego as represented by the patient in his prehypnotic state. Interview content, as well as Rorschach and Thematic Apperception Test (TAT) productions, differed discernibly in the three roles. The controversial question of a parallel between hypnotically induced multiple personality and hysterical dissociation is not answered by Leavitt's study.

Sweetland (79) attempted to induce an experimental neurosis by instructing subjects under self-hypnosis to behave as depressed or as hypochondriacal persons. He obtained satisfactory rises in the Minnesota Multiphasic Personality Inventory hypochondriasis scores, which were essentially the same as scores for clinical cases; changes in depression scores were satisfactory, except for the F, Pd, and perhaps the K scores. Wolberg (80) reports the induction of posthypnotic conflict in six subjects, three of them patients, in which the physiological components of anxiety behavior were prominent.

## Stress

Stress as a factor in producing behavior pathology receives increasing attention. Laboratory investigations of reactions to intense stimulation and to personal conflict continue to be rewarding. Supplementing the earlier reports from U. S. Army and Navy sources in *War Medicine* (81) and *Military Neuropsychiatry* (82), are a number of significant papers on combat stress, most of the recent ones describing the consequences of stress in the European theater of war. These are summarized in the reports of the *Inter-Allied Conferences on War Medicine*, some of which are referred to next.

By 1943 the Royal Navy estimated that 10 to 15 per cent of its hospital beds should be set aside for neuropsychiatric cases. Most of these, according to Curran (83), were anxiety states and depressions, complicated or prolonged by hysterical or escape mechanisms. Figures from one neuropsychiatric unit show that a primary diagnosis of hysteria was made in 15 per cent of the ratings but in only 4 per cent of the officers. In one representative depot, 90 per cent of the new cases could be kept at some form of duty; 75 per cent of inpatients in one NP unit were returned to some form of duty. Craigie (84) reports that in the Middle East Force, two large and unselected samples of neurosis cases showed 71 per cent anxiety and 10 per cent hysteria in the navy, 53 per cent anxiety and 18 per cent hysteria in the army. Percentage of battle neuroses was much higher in the navy. Forty per cent of the neurosis cases had a markedly bad previous history, and 20 per cent of them had suffered severe breakdowns in civilian life.

Curran (85) deprecates creation of a category called operational strain; psychological casualties in the Royal Navy do not often occur in the field, but rather after combat. Sudden alteration of tension, including tension reduction, is an important condition of succumbing to stress. Only 26 psychiatric casualties were known in 1940 in the submarine service; and many of these could be attributed to incidental factors, such as domestic difficulties. The incidence of neurosis in 8,905 men afloat corresponds to an annual rate of psychiatric illness of 4.3 per thousand; while the annual rate of psychiatric invaliding for the entire navy was 3.8 per thousand.

Rees (86) estimated that 30 per cent of admissions to British military hospitals in the unsuccessful Norwegian campaign were cases of battle neurosis; whereas in the first expedition to Benghazi, which was a war of

movement and successful, the figure was as low as 2 per cent. Likewise, Craigie (84) noted that the highest rates of neurotic breakdown in the Middle East Force occurred during and following evacuation from Greece and Crete. The single most important factor in producing battle stress was continued dive bombing in the relative absence of protection, and it was enforced inactivity under such conditions that was "particularly devastating." In one large series of neuroses, 64 per cent of anxiety states and 27 per cent of hysterical disorders were directly related to battle stress.

The most striking positive correlation found, in a statistical study of neuroses in an RAF bomber group, reported by Reid (87), was that between casualty rate and incidence of neurosis. Realization of hazard was far more important than cumulative fatigue. Also significant was the relationship between incidence of neurosis and proportion of inexperienced crews in a formation. No decisive correlations were demonstrated between incidence of neurosis and frequency of sorties, or evidence of physical disease.

Wright (88) reports a study on operational stress in highly-selected, successful bombardment fliers of the U. S. Eighth Army Air Force. Key factors in stress endurance were the need for conformity to the standards of squadron and crew, strong identification with and importance in the bomber group. Factors inducing anxiety were: the fact that survival was improbable; close-up, direct experience with catastrophe; deprivations, restrictions, and breaking up of powerful group ties; inability to escape or fight back in action; apparent undependability of authority; sense of responsibility and guilt over loss of crew members. Since virtually every man experienced anxiety, accepted it with remarkable insight, and continued to fly effectively with the symptoms, Wright questions the adequacy of the term "neurosis" in such a context. Analysis of the records of 1,500 psychotic soldiers brought out no essential difference between war-precipitated and peacetime psychoses (89).

Three studies of self-inflicted injury deserve mention. Fidler (90) examined 50 cases of self-inflicted gunshot wound among British military personnel. Of these, 22 were accidental, 23 doubtfully accidental, and 5 confessedly deliberate. Most such cases, whether accidental or deliberate, Fidler interprets as early stages of exhaustion neurosis. Five hundred instances of unsuccessful suicide attempt, 100 of them studied intensively, form the basis of an analysis by Arieff, McCulloch & Rotman of age trends and etiological factors in suicide (91). Pseudo-attempts were most common in persons younger than 30. All psychiatric categories were represented, with depressive disorders contributing the largest number. Alcoholism as a precipitating factor was found both in this study and in one by Wallinga (92), the latter study based on 381 cases of attempted suicide. Previous suicidal attempts characterized one-sixth of these cases, and one-fourth of them had previous psychiatric diagnoses.

Using the Hardy-Wolff thermal stimulator, Schilling & Musser (93) found no significant difference in mean pain perception threshold between

a group of 71 neurotic patients and a group of 138 non-neurotic subjects. The neurotic group showed a significantly lower average pain reaction threshold; but the overlapping between the groups was so wide that the usefulness of this measure in differential diagnosis is dubious. These results are essentially the same as those reported for 50 psychoneurotic patients and 56 controls in an earlier paper by Chapman *et al.* (94). Diminished pupillary response to pain, light, and muscular effort in 15 per cent of a group of 343 schizophrenics, as contrasted with 1 per cent of 100 normals, is related by May (95) to other evidence of hypo-reactivity shown by schizophrenics, to the lack of normal emotional reaction to the outer world, and to the patient's apprehension, suspicion, and timidity.

Malmo & Shagass (96) studied the EEG's, respiratory responses, muscle potentials, heart rate, and GSR's of 10 control subjects and 75 anxious patients when a series of painful stimulations was administered with the Hardy-Wolff thermal stimulator. Degree of physiological disturbance under this stress was found to be related to severity of anxiety, with the clearest correspondence occurring in measures of striate muscle activity. The more anxious the patient, the more likely he was to overreact to pain stimulation. Utilizing the same method in another study of 74 unselected psychiatric patients, Malmo & Shagass (97) found that patients with cardiovascular complaints showed significantly elevated heart rate, heart rate variability, and respiratory variability under stress; while patients who complained of head and neck pains showed heightened muscle potential scores.

## Frustration

A monograph on frustration by Maier (75) is noteworthy as a systematic attempt to apply concepts derived from animal experiments to human behavior pathology. Largely on the basis of his own experimentation with abnormal behavior in rats, which is summarized in Part I of the book, Maier questions the traditional analysis of behavior in terms of goal-directedness. A mechanism or process of frustration, ordinarily dormant, is aroused under certain circumstances and instigates reactions which differ from motivation-induced behavior. Frustration-instigated activity includes many reactions which are of importance to the behavior pathologist, such as fixation, aggression, and regression. Throughout the latter half of the monograph, Maier interprets a number of clinical and experimental studies of pathological behavior in human beings in terms of this hypothesis and advances a general theory of neurotic behavior.

Sherman (98) used experimentally-induced failure on digit-recall and maze-tracing to determine the frustration threshold of 75 normal, neurotic, and schizophrenic children and 40 normal, neurotic, and schizophrenic adults. His criteria of frustration were disruption of integrated activity, haphazard bodily movements, and changes in physiological reactions. In both children and adults, mean frustration level as measured by physiological change was highest for the psychotic group and lowest for the neurotic group.

Schizophrenic disorders as reactions to frustration are described by Nielsen & Thompson (99). They present case records to illustrate complete recovery achieved by patients who were able to overcome frustration through direct assault or sublimation; in the absence of such a reaction, continued frustration may result in chronic deteriorating schizophrenia.

### Anxiety and Behavior Pathology

In the 21st Maudsley Lecture, Ryle (100) discusses the problem of fear of disease, its etiology in anxiety which is often intensified by medical procedures, and its alleviation through proper education. Gitelson (101) presents case material to illustrate the way in which anxiety may intensify passive, dependent trends toward chronic invalidism, encourage repression and hence secondary symptoms, or stimulate denial both of anxiety and of the somatic disease itself. In a theoretical paper, Ferraro (102) analyzes "somato-psychic" factors in anxiety neurosis, paying particular attention to instances in which somatic change outlives the disappearance of conflict, and to those in which somatic change is primary, and psychogenic disturbance remote and secondary. Rudolf (103) reports that retrograde amnesia, in both candidates for officer training and men receiving pensions, is more likely to occur after accidents in which the patient has the opportunity of anticipating the danger.

An important collection of papers, most of them reprints, has appeared under the editorship of Alexander & French (104). In addition to Alexander's own papers on basic psychosomatic concepts, the book contains numerous clinical and theoretical studies of disturbance of gastrointestinal, respiratory, cardiovascular, metabolic, and skeletal function. A summary of the work on psychosomatic problems, particularly those aspects relating family pattern and social stratum to duodenal ulcer and thyroidectomy, which is in progress at the University of California Medical School under the direction of Ruesch, appears in the recent volume on personality edited by Kluckhohn & Murray (105). Ruesch's theory of social learning, with its implications for the relationship between infantile personality and psychosomatic disorder, is developed more fully in two theoretical papers (106, 107).

A study of 62 male patients suffering from duodenal ulcer, compared with 72 chronically ill nonulcer patients is presented in a monograph by Ruesch (108). His conclusions support the usual picture of the ulcer patient as one who is anxious, socially immature, hungry for affection, and unstable in marital adjustment. The data are unusually extensive, including results of medical, surgical, and radiological examinations, as well as psychometric and psychiatric evaluation. Klein (109) presents evidence against the existence of a specific personality pattern which predisposes to gastrointestinal illness. Sixty-three per cent of a group of 100 cases, referred to a gastroenterological clinic, were without intrinsic gastrointestinal pathology. This nonpathological population had essentially the same complaints, but more of them, and the same degree of chronicity as did the patients with identifiable intrinsic pathology. In general, the entire group

was thwarted or unhappy, gave a history of inadequate marital and vocational adjustment, and of emotional deprivation in childhood; but the personality patterns represented in the group were diverse and no specific relationships to gastrointestinal complaints could be discerned.

A group of 58 hypertensive subjects, studied by Wolf and his associates (110), showed major alterations in blood pressure while discussing personally significant conflict, as contrasted with a fairly stable pressure level during control periods. Only nine out of 15 nonhypertensive control subjects underwent similar changes. The authors report that in attitudes, as well as circulatory physiology, the hypertensive patients showed an inappropriate emergency mobilization pattern. Similar evidence that discussion of significant personal problems is related to urinary frequency or urinary retention is given in another paper (111).

Additional observations by Wolf & Wolff on a single patient with gastric fistula (112) provide information on the bodily changes in gastric hyperfunctioning and hypofunctioning. The character of emotional reactions accompanying each type of functioning depends upon the patient's attitude toward his own defeat in his adverse situation.

### Language and Thinking

A systematic interpretation of schizophrenic language distortions in terms of the principle of least effort is presented by Zipf (113). Degree of autism is defined as the degree of failure to discriminate between the virtually invariant language of physics and the variant language of culture. With this approach, Zipf next derives the theoretical characteristics of extreme autism and then proceeds to demonstrate that schizophrenics exhibit them. The results of statistical analyses of two patients' verbal output, totalling 65,000 running words, indicate that these schizophrenics are saying basically the same things in essentially the same way about an ever-increasing number of things. The patients also exemplify the principle of least effort in not bothering to check their correlations with observable facts, since to them the semantic interrelatedness of their own conceptualizations are of a more comprehensive reality than the language of either culture or physics.

White (114) compared the performance of matched groups of 20 normals and 20 early schizophrenics in three tasks. One task consisted of the interpretation of indistinct, blurred words; a second required the subject to sort words into two groups, one called "mine" and one "theirs"; the third task was that of making up a sentence which included an orally presented word. From the results, White concludes that (*a*) schizophrenics are unwilling to attempt interpretations but, when they do interpret, they show a need to generalize, even if the generalization makes no sense; (*b*) schizophrenics group into complex patterns, rather than into simple categories, and have difficulty shifting from one category to another; (*c*) schizophrenics perseverate and lack completeness of thought; and (*d*) schizophrenics avoid personal themes and prefer impersonal themes of morality, religion, life, and death.

Rashkis (115), using a word-sorting and number-sorting conceptual test, reports that groups of schizophrenics, general paretics, and cerebral arteriosclerotics tend to be behaviorally distinct from one another in a statistically significant manner. He interprets the thinking disorder in general paretics as a deficit both in performance potentialities and in organization, the disorder in cerebral arteriosclerotics as a deficit in performance potentialities but not in intellectual organization, and the disorder in schizophrenics as a deficit in organization but not in performance potentialities. The schizophrenics showed noncorrespondence between what they did and the explanation they offered afterwards.

Mintz (116) compares the bizarre statements of two normal adults, not thoroughly awake, with the speech of schizophrenics and suggests that the similarity indicates the presence of schizophrenic thinking in the repertory of normal persons. Moloney maintains that "motoric" and "glandular" language develops prior to laryngeal speech, and occasionally expresses total personality disturbance (117). In this context he presents a number of case histories to indicate that low back pain is a metonym for a total personality difficulty.

## Fantasy, Hallucination, and Delusion

The multiplication of projective techniques and the formal testing of psychoanalytic theory come together in a monograph by Blum (118) which presents a technique for the evaluation of certain psychoanalytic hypotheses regarding psychosexual development. Blum used a series of cartoons depicting the life history of a dog, "Blacky." Analysis of the responses of male and female college students to the pictures and to multiple choice questions about them supports 14 of 15 selected psychoanalytic propositions concerning stages in psychosexual development.

Shneidman (119) has used his MAPS technique (make-a-picture-story) in a comparative study of 50 male schizophrenics and 50 male nonpsychotic veterans. The test consists of 22 background pictures, against which the patient may place any of 67 cardboard figures. The patient is asked to arrange the figures as they might be in real life and to tell a story of the situation he has created. On the basis of his empirical findings, Shneidman presents a qualitative interpretation of the characteristics of schizophrenic fantasy. As compared with the controls, the schizophrenic patients show greater variability from picture to picture, tend to identify themselves more frequently with the test figures, arrange the figures with little interaction between them, show overinclusion by selecting many figures or more than one background, and often use the test materials inappropriately or in a symbolic or religious fashion. They typically inhibit themes of violence, place punishing figures in the pictures, fail to identify closely with the common male role, and devaluate female figures.

Some of these findings appear to be contradicted by the results obtained by Eron (120) in a study of Thematic Apperception Test productions of 25 hospitalized schizophrenics and 25 nonhospitalized college students, all male veterans of World War II. Eron's data suggest that the

properties of the stimulus-cards are more important than the specific behavior disorder in determining the themes of TAT stories. A number of characteristics generally assumed to distinguish schizophrenic fantasy themes occurred significantly more frequently in his control than in his schizophrenic group. Thus themes of moral struggle, death, and illicit sex behavior are more typical of the nonpsychotic than of the schizophrenic group. Likewise, confusion of the sex of the central character occurred infrequently in the total group, but more often, though not significantly so, among male college students than among male schizophrenic patients.

Despert (121) was unable to find evidence for "true hallucinations or delusions" in 106 normal nursery school children upon each of whom detailed observations had been made over a period of months. A few showed reiteration of fantasies with apparent belief in their reality. In 58 consecutive behavior problem children, only three reported hallucination, three delusion, and one both hallucination and delusion; none of these was under the age of seven and one-half years. Delusional and hallucinatory experience in psychotic children, above the 10 to 11 year level, were similar to those of adult psychotics, but much simpler and less systematized. In younger psychotic children, the content was difficult to elicit, but frequently it was repressive or represented identification with animals.

The development of fantasy and hallucination in wartime sailors, adrift for long periods in an open boat, has been described by Critchley (122). After the excitement over rescue from the water has died down, the men develop an irritable, mildly depressive apathy, which is punctuated by peaks of elation and despair as prospects of rescue approach and recede. Heightened suggestibility is common; one man suggests going below for some tea, another joins him, and both step off the boat to their death. The inevitable daydreaming of the survivors leads to fantasy-building and to active, vivid hallucination in which several may join. After final rescue, persistent psychiatric incapacity was found in only 50 out of 285 shipwrecked sailors.

Wagener (123) gives a critical review of reports on visual hallucinations, from the standpoint of the ophthalmologist. He concludes that the mere occurrence of any specific type of visual hallucination gives no clue as to its origin. The common assumption—that when a person with brain disease hallucinates visually in some particular way, there must be direct involvement of a specific cortical segment—Wagener dismisses as over-simplification. Seitz & Molholm (124) report that a group of 20 schizophrenics with auditory hallucination have a significantly lower mean percent of imagery than either a group of 20 schizophrenics without auditory hallucination or a group of 114 normals (student nurses). The same relationship held for a comparison of the normal group with 10 patients, who had recovered from alcoholic hallucinosis with marked auditory hallucination. Imagery was determined by the use of a "list of concrete imagery," consisting of 130 words and phrases to which the sub-

ject responded by reporting his initial image. These results, together with the findings of other workers in the past decade, suggest that deficient auditory imagery may be one factor responsible for auditory hallucination.

Gould (125) compared groups of state hospital patients with (N = 64) and without (N = 36) auditory hallucination, using electromyograms of vocal musculature with leads on lower lip and chin. He found a significant relationship between hallucination and increased muscle potential. Eighty-three per cent of the hallucinating patients showed increased potential as contrasted with 10 per cent of the nonhallucinating, while a heterogeneous group of 100 normals showed increased potential in 28 per cent. Pfeffer (126) found that, in abrupt withdrawal of morphine in three schizophrenic addicts, acute anxiety developed and, with this, an increase in the vividness of hallucination and delusion. In a comparison of ordinary bromide delirium and what he regards as bromide schizophrenia, Levin (127) reports that the latter showed characteristic self-reference in hallucination and delusion while the former did not.

A distinction between olfactory hallucinosis in nonpsychotic neurological patients and olfactory hallucination in psychotic psychiatric patients is made by Paillas, Alliez & Tamalet (128). According to them, the nonpsychotic patient recognizes the unreality of his olfactory symptom which remains isolated and meaningless; in only one out of 15 cases reported was a delusional interpretation built around the hallucinatory experience. Psychotic patients, in contrast, built the olfactory hallucination into their delusional systems and gave evidence that the symptom was deeply involved in their emotional life. Only this latter, they conclude, deserves to be called hallucination.

## Miscellaneous

In a comparative study of 10 nonhospitalized adult epileptics and 10 matched diabetic patients, Lisansky (129) was unable to substantiate the assumption that a typical personality picture distinguishes convulsive patients from other sick or partially disabled persons. Petrie (130) found no significant differences in tests of repression and suggestibility between a group of 34 hysterical and a group of 38 dysthymic patients; in both groups, repression and suggestibility and repression and neuroticism were positively correlated. Brady (131) found no significant differences in suggestibility or in persistence between a group of 213 feeble minded and one of 200 epileptic subjects; but both were less suggestible and more persistent than groups of neurotic patients whose scores have been reported in the literature. Arcieri (132) measured suggestibility by the deviation of a subject's drawing from the reproduction of a previously seen object toward a form suggested later by the experimenter. Deviation was greatest for a group of 17 neurotic and least for a group of 19 schizophrenic patients, with the 20 normals occupying a middle position.

As a contribution to the understanding of alcoholism, Karpman (133) presents three detailed case studies of alcoholic women, including verbatim material from psychoanalytically oriented therapeutic sessions.

Wallen (134) found a significantly higher mean food-aversion score in a group of 227 Marine recruits, about to be discharged for psychiatric reasons, than in a group of 789 randomly selected recruits.

By separately recording first admissions and readmissions to a state hospital, and excluding the old age psychoses from consideration, Israel & Johnson (135) found that 70 per cent of new patients are discharged. Of the patients who returned to the hospital, 50 per cent were again discharged and succeeded in remaining in the community. Menninger (136) presents a valuable compilation of statistics relevant to psychiatric problems which includes data on delinquency, unsuccessful marriage, physical disability, minority intolerance, and industrial unrest. Hyde & York (137) outline a variant of sociometric analysis that is suitable for use on hospital wards for the recording, graphically and in time sequence, of selected aspects of social behavior.

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# CLINICAL METHODS: PSYCHODIAGNOSTICS[1]

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The recent publications in the area of psychodiagnostics emphasize quite overwhelmingly the importance and potential scope and richness of projective techniques. This interest accounts for the large number of overviews, commentaries, and critiques of projective techniques, the varied and ingenious new projective devices, and the new applications of old devices which have been proposed. The freedom for idiosyncratic and idiomatic expression allowed the subject by projective techniques, in contrast with objective inventory-style personality tests, appears to be a major advantage. This allows the clinician to observe "the personality in action," so to speak, so that interpretation need not be limited to inference from scores or statistical recapitulations of performance. The qualitative aspects of performance on objective tests receive similar emphasis, so that the literature in this area seems to be becoming much more "clinical," much more "qualitative," and much less narrowly classification-oriented. In addition, the objective numerical scores and patterns achieved by a patient are no longer treated merely as indicators of his standard score or percentile status with respect to some specified clinical or normal group (15). Rather, they increasingly serve as signs of strengths and trends in various functions and are interpreted dynamically; that is, they are integrated and combined to provide the clinician with an understanding of the manner in which a patient's needs, habits, values, and the like articulate to produce his total personality. While this frees the clinician from the arbitrary restrictions imposed by tables of norms, it imposes great responsibilities upon him in the role of interpreting and discriminating instrument. This role, of course, is one that able clinicians have always had; but only within the past few years has this been made sufficiently explicit. Paralleling this, we find wider emphasis upon varied and intensive clinical experience in training programs and a distinctly new flavor in recently published texts and manuals (7, 15, 21, 60, 73, 77, 85, 87). Definition and explanation by case illustration and by test protocol analysis appear to be a favored mode of presentation in order to help the clinician acquire the necessary sensitivities to respond reliably and selectively to the wealth of data emitted by patients upon the occasion of psychodiagnostic study. Quantitative data have not disappeared, but the emphasis has shifted (64).

Tests provide for more than just extended observation. They also have specific diagnostic implications in a more formal sense. For example, tests such as the Rorschach are thought to reveal structural aspects of personality, in contrast with the need or drive aspects revealed by the Thematic

[1] This review covers the period from March, 1948 to July, 1949.

Apperception Test (TAT). The literature abounds with signs proposed as indicators of this or that trend or characteristic. In fact, the classical and formalized aspects of test interpretation for both projective and objective tests are weighted heavily with discussion of signs, though the ultimate value of such an atomistic approach is controversial. As both the tests and the test interpreters contribute to psychodiagnosis, inquiry into their respective and independent contributions is justified and should be emphasized more than it has been in the past.

## Projective Tests

Controversy here centers largely around the problem of validity and questions of scoring and interpretation. There seems to be general agreement that projective test results are rich in the sense of providing many fruitful hypotheses for diagnostic formulation, therapy, and personality research. Agreement also seems general that projective devices provide more in these areas than do objective tests, though this point is usually carried largely by implication. New scoring methods and refinements or clearer restatements of older schemes have been suggested as aids in ordering this wealth of material (12, 85). With respect to the TAT, Tomkins recognizes its ambiguous validity and reliability status (90). Rosenzweig, however, feels that the absence of formal group norms for this test may not be crucial, since each patient supplies his own individual norms in the course of creating his protocol (71). Internal consistencies and inconsistencies, idiosyncrasies in perception of the world, and the like generally serve a normative purpose. Sarason recognizes the necessarily subjective and intuitive character of TAT interpretation, but emphasizes that this does not remove the process from the realm of scientific inquiry and verification (76). Frank, in his chapter on validity, recognizes the need of definitive studies of projective test validity, but quite properly stresses that the traditional techniques customarily employed in validity and reliability studies may be inappropriate for this purpose (26). Krugman (53) and Wittenborn (95, pp. 133–34) make a similar point in their reviews of the Rorschach. The Rorschach and TAT sections in *The Third Mental Measurements Yearbook* (74; 95, pp. 206–7) reveal quite accurately the present somewhat indeterminate validity status of these techniques, though Kutash, in the Rorschach Research Exchange, presents a slightly more optimistic picture (55). Rotter, in another discussion of the present status of the Rorschach, mentions the paucity of crucial new validity data as well as the variability and inconclusiveness of the data now available, pointing out that this technique has not yet achieved the status of a dependable criterion device, though it often is so used (68). He also presents a pertinent discussion of some of the methodological shortcomings and difficulties in the "blind analysis" and matching techniques so popular in validation.

The projective methods still seem to rest largely on the evidence provided by a comparatively few small-scale studies done over a period of years and on the empirical, but only illustrative data furnished in testi-

monial and case report form by clinicians. The absolute number of these small-scale studies is considerable, but they seem surprisingly few when one considers the amount of projective test activity and the range of claims made for the projective tests. The most encouraging aspect of this state of affairs is the growing recognition that projective test results imply propositions that are testable and that intuitive and subjective clinical interpretation, for all its delicacy and subtlety, is not so precious or tender that it will decay and die when subjected to scientific scrutiny.

In addition to a number of case-illustrated discussions, several rather important large-scale research reports on the Rorschach have appeared within the past year. Bühler, Bühler & Lefever have made the first report on their Rorschach Standardization Project (12). This deals with the derivation and initial cross-validation of a "Basic Rorschach Score" on a total sample of 518 subjects. The score is a numerical value designed to indicate degree of personality malfunction or disintegration, ranging from "adequacy" through "conflict" and "impairment or defect" to "reality loss." The initial derivation of item weights was based on the performance of a group of 207 persons, including cases diagnosed as schizophrenia, psychopathic personality, and psychoneurosis (various types) as well as some organic patients and some normal subjects. The scoring scheme was then cross-validated on 161 new cases similarly distributed throughout the categories described above. The weighting system seemed to hold up fairly well in cross-validation. Analysis of variance of the scores obtained by the cross-validation cases indicates that the different diagnostic groups were significantly differentiated by the Basic Rorschach Score.

In one sense, this study depends upon psychiatric diagnosis as a criterion. Though the psychologist who did the testing also participated in the conferences wherein criterion diagnoses were determined, the authors tried to keep these diagnoses completely independent of the Rorschach findings (12, p. 5). The major diagnostic categories were considered as representing an hierarchy of illness or malfunction, with schizophrenia at the "reality loss" end of the hierarchy and normality classified as "adequate." The normal and schizophrenic groups for the total sample were separated completely. The implication of the Basic Rorschach Score depends heavily upon this fact. The authors also discuss a number of hypotheses concerning personality and indicate that a future report will deal with differential diagnostic indicators. This approach to the problems of Rorschach interpretation indicators seems to have promise. The report is quite elaborate, though still in preliminary form. It is unfortunate that space limitations preclude more detailed review here, because some Rorschach experts may feel that the "sign approach," Bühler's special method of administration, and some of the dynamic interpretations offered in the report are controversial (13). The report, however, indicates what the Rorschach can do when used and interpreted in this way, though it probably cannot be considered to show what the Rorschach does do when used and interpreted more conventionally.

In another noteworthy study, Hertz cross-validated the Rorschach suicidal configurations she proposed early in 1948 (36, 37). At that time she described 10 configurations (based on total Rorschach performance rather than isolated signs) which differentiated between suicidal and nonsuicidal persons. The original sample consisted of 229 clinic patients and 96 normal subjects. Six or more of these configurations characterized the records of 92 per cent of the suicidal patients, while the records of only 35 per cent of the nonsuicidal patients and 3 per cent of the normal subjects contained this number of configurations. The cross-validation sample consisted of 178 clinic cases, including adults and adolescents in the psychoneurotic, psychotic, organic, and normal categories. The criterion classification, as in the original study, was based upon evidence of any ideas or talk of suicide, suicide attempts, or successful suicide in the case record or correspondence. Every effort was made to keep the test results and the criterion completely independent. Hertz recognizes that her criterion leaves much to be desired as an indicator of suicidal trends. Under these circumstances, however, the consistency and significance of her cross-validation results testify to the durability and significance of her configurations. Agreement between Rorschach diagnosis of suicidal trends, based on the presence of five or more of the configurations, and the independent criterion occurred in 84 per cent of the cases. This cutting score misclassified 17 per cent of the suicidal and 15.7 per cent of the nonsuicidal subjects. These percentages have some practical significance too, since her population was a total sample of consecutive cases rather than a preselected sample equally divided between control and experimental subjects, as in most studies.

The configuration approach used by Hertz, together with the clinical interpretation and judgment it required, closely resembles the way in which the Rorschach usually is employed. This inevitably confounds the performance of the test and the performance of the tester-interpreter. Hertz actually cross-validated the combination. As clinical practice almost universally requires a similar confounding, the experimental investigation of this combined performance is highly appropriate. Hertz has described the configurations explicitly. Other workers should be able to use them. Further experimentation with different test-interpreters will be required, however, before the independent contribution of the Rorschach alone can be definitively established.

Unfortunately, a complete report of Hughes' investigation of Rorschach diagnostic signs (38) was not available at the time this review was written. He has, however, reported highly encouraging data on the clinical application of some signs of organic pathology (39). He indicated a cutting score which correctly identified 82 per cent of the organic and 99 per cent of the nonorganic cases in a diversified sample of 218 cases, a great improvement over the performance of the Piotrowski signs (65) for the same sample. These results are partially in line with those reported by Aita, Reitan & Ruth (1). The satisfactory performance of the Rorschach in this area is still not completely substantiated, however. Daw-

son, in a study of 24 verified organic cases and a matched control group of 24 nonorganic cases with psychoneuroses, found that the best of four Rorschach interpreters diagnosed 75 per cent of the control but only 52.5 per cent of the organic cases correctly (20). This agreement between "blind diagnosis" from the Rorschach and the carefully verified criterion was only of borderline significance ($p$ between 0.05 and 0.02). The other judges performed below this level of accuracy. Blind Rorschach diagnosis, if maintained at this absolute level of accuracy, could achieve more convincing statistical significance in a substantially larger sample. The frequency of correct judgments, however, rather than the statistical significance of group differences determines the practical utility of a differential diagnostic method. Dawson's findings should not be "shrugged off" as inconsequential because of his small sample. Unfortunately, the three studies are not directly comparable. Different signs were employed, different judges contributed diagnoses, and the cases probably differed considerably. Cross-validation of the signs and methods employed in each, on the data of all three samples, should be revealing.

Schafer & Leitch report favorably on the performance of the Rorschach and TAT in an exploratory study of the application of a battery of tests to nursery school children (78). The criterion of adjustment was based on psychiatric ratings, historical material, and observations made without knowledge of test results. Whether or not knowledge of the criterion ratings was available to the test interpreters cannot be determined from the report. The battery, Rorschach, TAT, and Stanford-Binet, discriminated between the three criterion levels of adjustment quite satisfactorily. Of the tests, the Rorschach seemed most fruitful to the authors. Wishner reports evidence indicating the value of Beck's Z and the Rorschach signs R and W in the determination of level and quality of intellectual performance in a sample of 42 diagnosed psychoneurotic patients, with Wechsler-Bellevue performance as a criterion (94). Interestingly enough, F+ percentage did not correlate significantly with any of the Wechsler-Bellevue scores.

Other Rorschach studies on color shock (57, 92), group testing (63), prediction of extramural adjustment of mental defectives (83), Roe's work on professional groups (70), relations between Rorschach and objective personality tests (5, 16, 88), and the influence of hypnotic suggestion on movement responses (56) illustrate the wide range of application and the varied areas into which research on this test may go. Some reports, such as those of Kurtz (54) and Lazarus (57), are negative and discouraging, some are inconclusive or cautionary (83, 96), but some are positive. Garfield reports agreement between Rorschach diagnosis and clinical diagnosis in 79 per cent of his sample of 58 patients (27). The test results contributed to the criterion diagnoses; therefore, Garfield's study by no means establishes the validity of the Rorschach for classical diagnostic discriminations. Experimental designs which do not guard against criterion contamination and reports which are insufficiently detailed to allow the reader to determine whether such contamination has occurred

appear with sufficient frequency to make it hard to evaluate the present validity status of projective techniques.

Some of the publications on other projective techniques give little more than lip service to the necessity for validation. Machover, in her discussion of drawing the human figure as a projective test, presents nothing by way of validity beyond the statement that the principles of interpretation she describes "have received repeated justification and verification in clinical usage" plus some references to case files with drawings and clinical notes, and some "blind matching" studies which came out "much better than chance" (60, p. 25). Yet the text suggests specific interpretations for a wide variety of idiosyncrasies in drawing, some of which can, and probably will, be tested experimentally. In the event that other experimenters get negative or inconclusive results, which they may just by virtue of sampling errors or similarly nonclinical factors, this projective technique which may have considerable potential value might be seriously embarrassed.

A similar situation exists with respect to Deri's long awaited report on the Szondi test, a unique device consisting of 48 photographs divided into six sets of eight pictures each (21). Each set contains one photograph of a person belonging to each of eight diagnostic categories. Interpretational inferences are based on the pattern of preferences and antipathies a subject shows for the pictures, the changes in this pattern upon retesting, and associations to the pictures. Deri explicitly states that her interpretations do not depend upon Szondi's rather original notions about genetics, but rather they lean heavily upon Lewinian theory and have their empirical roots in her own extensive clinical use of the test. But such details as frequency of agreement between Szondi interpretations and independent criteria do not appear in the book which is filled largely with discussion and case illustrations. The methods she suggests for interpretation seem original and profitable to explore. As some of the Szondi interpretations have direct consequences in behavior, experimental verifications will no doubt appear soon. The other report on the test is at a case discussion level, however (14).

Wolff's book on handwriting and expressive movement stands in a class by itself (97). It is noted here because of the obvious implications his hypotheses and methods of analysis have for future developments in psychodiagnosis. These seem fruitful enough to encourage research by others in this somewhat neglected area. He includes no large-scale clinical validity studies, in the formal sense, but does briefly describe a number of small experiments which lend credibility to his assertions. The correspondence between changes in graphic productions and vicissitudes in the lives of a number of historical and well-known individuals, plus the individual consistencies these graphic productions show, is particularly encouraging and deserves attention.

New techniques such as Blum's Blacky Test (10), the MAPS test (48, 81), Rosenzweig's Picture Frustration Test for children (72), Sherriffs' Intuition Questionnaire (82), and others represent interesting and

probably fruitful variations in psychodiagnostic method. More widespread application of the older devices such as Van Lennep's Four Picture Test (91) and the Mosaic Test (18, 59) in this country, together with cross-validation and clinical studies of all of these, will no doubt add greatly to our resources for personality study.

Psychophysical and factorial studies of temperament, designed along the general lines intimated by Thurstone (89), would employ many of the essentials of the projective method. Researches on such techniques may be quite profitable if handled in a clinically and statistically sophisticated fashion. Another technique, the psychodrama, seems to be moving into the area of diagnosis as a "situations" test. Bronfenbrenner & Newcomb (11) describe the essentials of the "situations" test as employed in the Veterans Administration Project on Selection of Clinical Psychologists (50). Soskin (84) has recently completed an intensive analysis of the ratings based on this test. He found that pooled ratings based on situations alone correlated significantly with ratings of the same traits based on complete case and test data, though their reliability tended to be lower. Rotter & Wickens (69) found a fair degree of inter-rater agreement for a "situations" test. Both reports discuss factors influencing reliability of ratings from situations. If the "situations" technique can be refined, it should be one of our most powerful tools for the investigation of personality.

### Objective Tests

The years have not treated these techniques in a kindly manner. Though the standard, individual intelligence tests such as the Stanford-Binet and the Wechsler-Bellevue have proved durable and useful, both the validity and clinical utility of inventory-type personality tests have remained equivocal. No doubt part of this can be attributed to intrinsic defects such as the restrictions they place on the subject and the difficulty of communicating meaning, intent, and feeling by way of structured and verbal modes. New fashions in clinical practice and the increased popularity of dynamic personality theories have contributed as well. In addition, the manual writers have tempted critics by being very optimistic and explicit in describing the functions of the tests. Rather clear-cut and literal research on inventories is easy, though sophisticated and dynamically oriented research is not. By contrast, the more complicated projective techniques do not lend themselves readily to such an attack. The shortcomings of inventories thus stand clearly revealed.

Ellis, in an earlier review of the status of objective, inventory-type personality tests, concluded that they were of distinctly dubious value for distinguishing between groups of adjusted and maladjusted individuals (23). Some of the studies he surveyed reported significant differences between groups, but for the most part the tests did not demonstrate their usefulness in individual diagnosis. Ellis' technique of counting the numbers of negative, positive, and equivocal studies in order to ascertain the consensus about each inventory oversimplifies the problem. It implies that the various studies somehow deserve equal weight. This is mislead-

ing. One good study is more convincing than 10 poorly designed and executed experiments. Ellis' general conclusion comes uncomfortably close to the truth, however. In a subsequent review of the validity of personality inventories in military practice, Ellis & Conrad came to the somewhat more favorable conclusion that these devices made a definite contribution to psychiatric screening (24). The review contains a detailed analysis of some of the criterion and sampling problems which hinder the performance of screening devices. The authors present a particularly pertinent argument concerning the statistical inadequacy of some of the studies in which the normal and the abnormal, or criterion groups, were approximately equal in size. This condition leads to inflated biserial correlation coefficients and misplaced confidence since this equality generally does not obtain in the unselected populations which ultimately are screened. The problem can be illustrated clearly in connection with contingency studies on the frequency of correct test diagnoses. For example, let us assume that a particular personality inventory actually has a false positive rate of 10 per cent and a false negative rate of 30 per cent (the obtained percentages for some tests seem to be approximately of these magnitudes). This would mean that 90 per cent of the normal population and 70 per cent of the abnormal population could be correctly diagnosed, and 10 and 30 per cent, respectively, incorrectly diagnosed by test scores alone. In a study of the diagnostic effectiveness of this hypothetical test with 100 known abnormal and 100 known normal persons as subjects, the over-all agreement between test diagnosis and the criterion would be 80 per cent, a gratifying result. Furthermore, the odds on being correct with a test diagnosis of abnormality would be 7 to 1 while the corresponding odds for a test diagnosis of normality would be 3 to 1 (see Table I). Is

TABLE I

ILLUSTRATIVE CONTINGENCY TABLE FOR ACCURACY OF DIAGNOSIS WITH GROUPS OF EQUAL SIZE

| | | Test diagnosis: Normal | Test diagnosis: Abnormal | Size of group |
|---|---|---|---|---|
| Criterion diagnosis | Abnormal | 30 (30 per cent) | 70 (70 per cent) | 100 |
| | Normal | 90 (90 per cent) | 10 (10 per cent) | 100 |

this a good test for screening? Usually the normal group is much larger than the abnormal or criterion group, especially in most psychiatric screening situations. For the argument, let us assume that the test is used to screen a population of 1,000 cases. Nine hundred of these cases actually are normal and 100 are abnormal according to our hypothetical criterion. If the test continues to yield the same percentages of false positive and false negative diagnoses, the over-all agreement between test diagnoses and the criterion would be 88 per cent and the odds in favor of being

correct with a test diagnosis of normality, 27 to 1. But the odds against being correct with a test diagnosis of abnormality would be 9 to 7 (see Table II). Again, is this a good test for screening? Automatic and "blind"

TABLE II

ILLUSTRATIVE CONTINGENCY TABLE FOR ACCURACY OF DIAGNOSIS WITH GROUPS OF UNEQUAL SIZE

| | | Test diagnosis | | Size of group |
|---|---|---|---|---|
| | | Normal | Abnormal | |
| Criterion diagnosis | Abnormal | 30 (30 per cent) | 70 (70 per cent) | 100 |
| | Normal | 810 (90 per cent) | 90 (10 per cent) | 900 |

test diagnoses of abnormality under these circumstances would be wrong more often than right! Adjustment of the cutting score can reduce the frequency of false positives considerably, but at the cost of increased false negatives. The potentialities of a screening test must be evaluated not only in terms of false positive and false negative rates but also in terms of the expected proportionality between the groups to be separated by the test.

The general problem is not limited to the screening situation either. It enters the clinical study of individual cases with either objective or projective tests. The utility of a test or method with respect to a particular diagnosis or a particular formulation is not adequately defined by its false positive and false negative rates for that diagnosis, but also depends upon the relation between those rates and the relative frequency with which the particular criterion condition or diagnosis actually occurs in the clinical population. This general argument receives emphasis here because it is so often overlooked.

One outstanding development in the objective test area has been the postwar research in abbreviated screening tests. Hunt and his co-workers have reported a series of large-scale projects (42 to 46, 52). They devised and standardized several brief intelligence scales and restandardized parts of other intelligence tests for use in screening naval recruits. They present evidence to show that their CVS intelligence test (comprehension, vocabulary, and similarities) can differentiate significantly between schizophrenic, normal, and mentally deficient individuals on the basis of the pattern of discrepancies between the three parts of the test. [Also see (3).]

Though the CVS application of the discrepancy or psychometric scatter principle to differential diagnosis has been rather fruitful, recent studies on the diagnostic utility of the psychometric scatter patterns of its parent test, the Wechsler-Bellevue Adult Intelligence Scale, have been less encouraging. Levine, in a direct and forthright investigation of the diagnostic utility of the schizophrenia patterns proposed by Wechsler,

found agreement of only borderline significance between "blind" diagnoses from these patterns and independent criterion hospital diagnoses (58). Though he did not regard this as necessarily invalidating the patterns, he concluded that their utility was doubtful because they were wrong so frequently in diagnostic implications. Though unreliability of the criterion may have influenced his results, his conclusions are supported by the inconsistencies and variations in Wechsler-Bellevue scores for schizophrenic patients, as reported by others (28, 49). A study on Wechsler-Bellevue test-retest reliability, reported by Hamister, also helps to account for these equivocal results (34). Hamister found that the test-retest reliabilities of the different subtests of the Wechsler-Bellevue differed considerably, and he pointed out that, because of this, something more precise and rational than the method Wechsler suggests is needed for determining the significance of subtest deviations. Wechsler's deterioration ratio, an application of scatter or pattern analysis to the detection of intellectual deterioration, enjoys a similarly ambiguous status. Though two reports affirm, on questionable grounds, the ability of this index to differentiate between brain injured and normal subjects (2,9), others indicate no significant relation between the ratio and either the intellectual deterioration supposedly accompanying psychosis (79) or the Shipley-Hartford scale for measuring deterioration (29, 61). The whole psychometric scatter approach to differential diagnosis needs much more investigation to determine its utility (47). Significant group trends are not enough. They promise rather than produce.

One of the most interesting reports on personality inventories describes the development of the K correction for the Minnesota Multiphasic Personality Inventory (MMPI) (62). This particular attempt to correct the profile for distortions introduced by the subjects' test taking attitudes has not, so far, clearly demonstrated its practical utility for standard differential diagnosis (41, 80). The difficulty, however, may rest with the magnitudes of the correction weights rather than with the logic underlying the correction. Some evidence indicates that the K score itself, in combination with other MMPI scores, may have clinical value for the detection of deception (17, 40). Recent reports suggest that the MMPI contains items which may be combined into scales having a wide variety of implications (4, 22, 30, 31, 93). Significant clinical correlates of K, which in one sense gets at attitudes toward the self, and of the other validity scales may also be isolated.

Several reports indicate the doubtful utility of the published and more or less widely used profile standards for differential diagnosis (41, 75). Since these profiles were based on group trends and since the criterion of standard hospital diagnosis leaves much to be desired, this outcome is hardly surprising. The report of Harris *et al.* (35) and earlier reports suggest that more sophisticated and dynamic interpretation of the MMPI may have clinical validity.

Here, as with the projective techniques, the popularity of the test seems to rest heavily upon the evidence provided by a few studies reported over a period of years and upon the personal and published

experiences of clinicians. The reviews in Buros (87) present a clear picture of the present controversial status of the objective personality tests.

Several new, though as yet essentially untried, objective tests have appeared recently, among them being the Halstead-Wepman Aphasia Screening Test (33), a systematic survey of language and symbolic functions. The problem of psychometric diagnosis of cerebral pathology has not really been solved yet, though a recent report on the Memory-for-Designs Test shows encouraging group differences (51). Some of Teuber & Bender's visual techniques (8, 86) and Halstead's experimental tests (32) may aid eventually in an area with cases where more traditional testing techniques have performed equivocally.

### The Clinician as a Psychodiagnostic Instrument

As intimated above, the utility of psychodiagnostic methods usually depends both upon the characteristics of tests and upon the skill, sensitivity, and accuracy of the interpreting clinician. As yet, we know little about the peculiarities and idiosyncrasies of the clinician as a psychodiagnostic instrument. We need to know more because literal, automatic interpretations from tables of norms have demonstrated their limited usefulness. Techniques making greater demands upon the acumen of the clinician have increased in popularity.

Even at its present stage of progress, the Veterans Administration Project on the Selection of Clinical Psychologists provides a rich store of provocative information on the clinician as a rating and interpreting "device" (50). Analysis of the ratings made by the assessment staff suggests, for example, that clinicians differ in ability to utilize different tests and in ability to judge various traits. The contribution made by a clinician's preferences and antipathies for different students to judgments of their competence (or vice versa) opens several new areas of personality study for investigation. Some evidence also indicates that projective test results, taken by themselves, create a somewhat less favorable impression about a person than appears justified on the basis of all available information about him, including personal contact (84). In the absence of good criteria, it is impossible to tell which of the impressions is more nearly correct, but there is little doubt that trends such as this have implications for validity research. And the relationships seem too systematic to be attributed entirely to unreliability of the rating scales and tests.

### Comments

Space requirements have limited this review to consideration of a few basic aspects of the broad field of psychodiagnostics. The emphasis here assumes that in this essentially applied field, the crucial problems at present largely concern validity, i.e., the relative frequencies with which test results imply correct and incorrect propositions about people. Detailed discussion of some of the theoretical aspects of personality study and some of the very important qualitative contributions the different instruments make had to be neglected for the time being. Similarly, many worthwhile reports and tests could not be cited.

The validity problem is not as simple as parts of the discussion might seem to suggest. Potentially worthwhile techniques can be seriously undervalued if unsuitable or unreliable criteria are used to study them. Classical psychiatric diagnosis may be such a criterion; the evidence suggests that it is unreliable (6). Personal validation (25) and its cousin, "clinical experience with a technique," have serious faults. Qualitative evaluations of behavior and personality trends are hard to state in reliable and communicable forms (50, 84). To validate one test with another helps very little unless one is interested only in the relation between the two. Real first-order criteria, criteria which do not demand that somebody rate or judge at some point in the process, are elusive indeed. Usually one is thrown back on judgments, ratings, formulations, descriptions, and the like. One line of attack on the criterion should include attempts to refine such judgments. It is not inconceivable that the introspections of clinicians as to their reactions to patients and to diagnostic or therapeutic situations might prove valuable. Possibly we can get some helpful suggestions for refinement from the methods of psychophysics. Another approach should be along the general lines described by Cronbach (19). This method is laborious but it does provide for ascertaining the agreement between criterion judgments and either global, or single item interpretations over a wide range of topics. We can find out only by trying.

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# CLINICAL METHODS: PSYCHOTHERAPY[1]

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The literature on psychotherapy has been steadily increasing in volume since the war, and in the past year more than 400 articles appeared. Because of limitation in space, only a representative sample of these articles can be reported. Those which were arbitrarily eliminated were simple descriptions of methods which have been previously described, illustrative case histories, recapitulations of well-recognized theoretical concepts, and references pertaining to work with children. The most feasible method of giving meaning to the extensive group of studies remaining appeared to be to point out certain rather noticeable trends and to illustrate them with the more significant studies. Unfortunately, even by this method, some entirely satisfactory studies cannot be mentioned.

## Theoretical Explanations of Psychotherapeutic Principles

Probably the most important contributions have been those attempting to develop new theoretical explanations of the process of psychotherapy. Most of these were attempts to explain the therapeutic process by means of one or another of the recognized systems of learning theory. Mowrer (1), for instance, proposes that psychotherapy must involve the process of unlearning of the techniques by which the patient has prevented normal emotional conditioning. Following the unlearning, more socially acceptable learning may take place. A somewhat similar idea is elaborated by Shaw (2, 3), who postulates that maladjustments represent behavior which has an immediately rewarding aspect and a remotely punishing aspect. In therapy it is necessary to make the cues of the punishing aspects of the maladjustment available to the individual, and this can be done through reducing his defense barriers and permitting examination of his behavior. The punishment then, in a sense, becomes more immediate. Following this process, it is necessary for positive relearning to occur to replace the maladjustive response. If the behavior is actually practiced, at least in symbolic form in the therapy, the relearning takes place. The counselor assists the client in this practicing process. Shoben (4) develops a variation in proposing that psychotherapy is a learning situation in which anxiety is eliminated by means of reconditioning. The stimuli which evoke anxiety (problems) are reinstated sym-

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bolically through the language process (Freud's catharsis) and paired with nonanxiety provoking stimuli provided by the warmth, security, and reassurance of the counselor. Miller (5) has attempted to equate Pavlov's concept of generalization with Freud's mechanism of displacement. The hypothesis has been tested by experiments with rats which were trained to fight each other, but in the absence of the fighting opponent struck at a celluloid doll in the cage. Control rats ignored the doll. This behavior was considered to be the equivalent of object displacement. In another experiment, thirsty rats which had learned to run a maze to obtain water, even though satiated with water, would run the maze faster and drink more water if they were motivated by hunger, pain, or fear than when not so motivated. This appeared to represent drive displacement. These generalized responses showed extinction and spontaneous recovery in the absence of appropriate drive or reward. Ruesch (6) describes a theoretical orientation which he terms a social learning theory for psychotherapy. The essential components involved are: primary needs, secondary needs, goal responses, instrumental action, exteroceptive cues, proprioceptive cues, goal reward, and non-goal reward. In his discussion he lists 17 principles which explain factors which inhibit therapy or learning, or which can be used to overcome patient resistance to learning.

A second group of theoretical discussions is that aligning therapy more with processes of perception. Estes (7) states that "when a contemporary, recurrent situation is clearly and consistently different from earlier situations . . . perceptual conflict is instigated." The corollary to this is that a satisfactory therapeutic relationship "induces a set of conditions in the client which permit and encourage the observing, thinking, and enlargement of social experiences which in turn bring about the reorganization of the handicapping self-other assumptions." A similar type of theory is that developed by Snygg & Combs (8, 9, 10) in which "internal" failure of adequate differentiation is the basis of neurosis. Therapy becomes a process of reperception of the self-concept and its relation to the environment. The concept of therapy as a reperception has been developed by Rogers (11), who contends that therapy can take place only in a nonthreatening environment. This he relates to Gestalt concepts, implying that the individual may perceive incorrectly because the stimulus can be distorted even before the subject consciously perceives it.

Along different lines, there is an interesting discussion by Domarus (12), who feels that anthropological investigations illuminate the environmental aspects of the psychotherapeutic process. He defends this view by pointing out the absence of the Oedipus complex and of neurotically produced psychosomatic ailments in certain island groups. He holds, therefore, that all psychotherapies must be based on an understanding of the culture in which the patient has lived.

Bordin (13) has proposed that psychotherapies could be analyzed in terms of numerous dimensions. In addition to analysis of directiveness

versus nondirectiveness, the investigator might consider other dimensions such as attention to clients' feelings as against intellectual solving of problems.

### Awareness of the Need for Research in Psychotherapy

A very noteworthy trend is increasing awareness of the need for research in psychotherapy. This is most characteristic of psychologists but is appearing also in the writings of psychiatrists. The psychologists visualize the work as experimental, with controlled variables, while the psychiatrists think usually of nosological reporting. In a symposium at a meeting of the American Orthopsychiatric Association, members of both professions discussed problems involved in an objective evaluation of psychotherapy. Kubie (14) made much of the problems of control and proposed a neurosis center for research purposes. This emphasis on the need for group co-operation in research is an important development. Shakow (14) described a very adequate hypothetical research project involving sound film recording, and groups of judges, including proponents and opponents of the technique, but not the therapist himself. At another round table of the same association, Rogers (15) proposed six types of investigations of psychotherapy, including correlations of physiological measurements with therapy, and comparative studies of methods. Kubie (15) held that basic to research in psychotherapy is agreement on essential units of illness to be treated and essential therapeutic processes to be evaluated.

Nineteen possible types of research on personality theory by means of analyses of psychotherapeutic interviews have been proposed by Snyder (16).

Among the persons most aware of the need for research in psychotherapy and the possibilities for doing such research are those who are interested in the nondirective method. Integrated research programs, organized at the University of Chicago, are described by Raskin (17, 18). In these studies, various researchers worked with a common group of phonographically recorded cases, analyzing different aspects of the material, and integrating their findings with those of other investigators. Rogers (19) indicates that in these researches the most important trends are in the direction of creating objective and verifiable descriptions of therapy, analyses of the attitudinal orientation of therapists, and analyses of the process in terms of evaluations of client attitudes.

### Research Studies on Psychotherapy

A most gratifying and wholesome trend has been the large number of completed researches on psychotherapy reported in the literature. The largest group of these either relates to nondirective therapy or utilizes the nondirective interview as a means of studying personality factors. Twenty-two such studies were published in the past year; seven are frank evaluations of the nondirective process. Carr (20), Cowen (21), and

Reader (22) evaluated the process in terms of changes in personality test scores before and after treatment. Cowen also worked out a rating system for analyzing follow-up interviews. Two of these studies obtained favorable findings, and one was negative. Hamlin & Albee (23) used the same technique for evaluating a control, or untreated, group as a check on Muench's positive findings (102) with a group treated by nondirective methods. Two studies, Seeman's (24) and Stroms' (25), used the interview analysis technique developed by Snyder (103) to follow-up on present developments in the nondirective method. The studies clearly show that in comparison with the earlier findings, the method has become considerably more nondirective in character. Interpretations and other directive elements are practically nonoccurring in cases treated at the University of Chicago. Using a different approach, Lipkin (26) analyzed the subjective posttreatment statements written by subjects in answer to a few leading questions about their reactions to their treatment. All but two of the 37 subjects expressed favorable opinions of the treatment, as might be expected.

A rather unique study was done by Thetford (27), who measured galvanic skin response and heart and respiratory rate of subjects during a frustrating situation before and after nondirective counseling. Reduction in emotional responses occurred in the group counseled, and not in a control group. Hardy (28) attempted to correlate personal traits of dominance, as measured by five tests, with nondirectiveness in the counseling of 25 student counselors. No satisfactory correlations occurred, a finding which was interpreted as possibly relating to the counselors' psychological sophistication in taking tests, and partly to the skewedness toward dominance scores in the group of counselors. Carnes & Robinson (29) found that it is not possible to use the amount of client "talk" in an interview as a criterion of counseling effectiveness. Actual changes in the maturity of the behavior reported by clients following their therapy were investigated by Hoffman (30). These were based on ratings by judges, for which a high reliability was determined. Kauffman (31) made a comparison of two methods of evaluating spontaneous or unstructured verbatim material such as counseling interviews. The methods were Raimy's analysis of degree of emotional involvement (104) and Dollard's Distress-Relief Quotient technique (105). He found that both methods were reliable ways of measuring changes in the client's attitudes, and that even though the methods seemed to be measuring somewhat different changes, these changes were closely correlated.

The next group of research studies consists of follow-up investigations of other treatment methods. Schofield (32) used the Minnesota Multiphasic Personality Inventory (MMPI) to evaluate treatment of the patients at the University of Minnesota Psychiatric Hospital. The tests showed no significant changes for persons treated in the outpatient department by means of about five interviews, or for patients who were diagnosed as psychotic but who did not receive shock treatment during

their month of hospitalization. Improved scores occurred for the psychoneurotic cases (inpatient) in the areas of hypochondriasis, hysteria, and psychopathic deviate, but not in depression. Improved scores occurred for the psychotics who had received about six shock treatments; the improved scores were on several of the prepsychotic signs. It is interesting that all of these patients had been discharged as improved.

An attempt to follow-up the treatment of 700 veterans was made by Feldman (33), who received replies from only 13 per cent of his group. They indicated need for further help, or concern over inability to secure psychiatric help. Since the size of the sample replying was so limited, it is difficult to interpret these findings as either encouraging or discouraging. Lewin (34) evaluated nonauthoritative treatment of juvenile delinquents by means of careful checks of the case histories and behavior during parole, and found that the treated boys improved socially and emotionally and had shorter parole periods.

A very significant group investigation directed by Hunt (35) has been the attempt to check the reliability of case workers' measurement of change or "movement." In view of what is usually thought about subjective judgments in diagnosis and case reports, it is interesting to note that Hunt finds his case workers able to rate their cases quite objectively in terms of carefully applied criteria of actual change in the case. He finds that this ability to make these ratings is closely correlated with the amount of training and experience the worker had has.

An attempt to check on the process of psychoanalysis by means of the MMPI, administered at two-month intervals, was conducted by Wiener & Phillips (36). The test findings tended to confirm the subject's reports of treatment, which were made without his knowledge of the test scores.

Dymond (37) conducted a study on the ability of subjects to predict what other persons would do under certain circumstances. The results indicated that empathy and self-insight are closely related.

There were six investigations which used the technique of reporting statistics on the percentage of cases considered cured after programs of special or general treatment. Although these studies show a wholesome effort to evaluate psychotherapy, the criteria of evaluation are so indefinite or so unmeasurable that the results are difficult to evaluate. As would be expected, the studies tended to show that individuals receiving therapy showed a greater incidence of cure than those not receiving it. Israel & Johnson (38) have proposed that many such statistics are given to unwarranted pessimism because of failure to isolate new cases from chronic cases. If new cases alone were evaluated, much higher incidence of improvement could be expected.

### Evaluation of Failures in Psychotherapy

Recently there has been some increase in willingness to admit failures in treatment and to try to determine why they occurred. Hoch (39) has

edited twelve contributions to the topic *Failures in Psychiatric Treatment.* In each of these articles an eminent therapist describes reasons for failures with his method. The reasons given can be summarized in general as being inappropriate diagnosis of the problem, inappropriate method of therapy, personal limitations in the client-therapist relationship, and intensity of background factors, which make the problem too difficult for psychotherapy. The problem in applying these principles, however, is that it is usually difficult to isolate their contribution, and no clear-cut criteria as to what makes it favorable or unfavorable have yet been determined for each. For example, there is no agreement as to whether psychotics should or should not be treated by certain specific psychotherapeutic methods. Each of the contributors to this book gives his impressions as to the sorts of cases which are untreatable by his method.

It is interesting to note that of this group of authorities only one was a social worker and none were psychologists. Two psychologists did make comments about two of the papers, and both of them strongly stressed the research aspects of this question of failure.

### Discussions about Nonmedical Therapists

In the past year one of the most surprising developments has been the number of discussions of the nonmedical therapist. More than 50 articles dealt with this topic. Eleven of these debate the pros and cons of whether the psychologist should practice therapy. Almost without exception, psychiatrists contended that they should not, except when they are supervised by psychiatrists, and also, that psychologists should be limited to work with mild or uncomplicated cases. Among psychologists the opposite opinion was not as universally held as might have been expected. A number were willing to accept the team-approach which usually means supervision by a psychiatrist. Some psychologists, however, hold out for complete independence but believe that a careful medical examination of the patient is important. About the best analysis of the situation is that of Shakow (40), who presents an imaginary dialogue between members of the two professions. Among other points, he suggests that most psychologists really prefer research work and regard that as their basic contribution. This point might be more true of academic psychologists. The question will surely not be settled for some years to come. Meanwhile, a committee of the APA (41) is developing for psychologists a code of ethics in various areas, including treatment.

While the debate has probably been precipitated by psychologists, other professions also are concerned. Eleven references relate to the responsibility of the minister as a counselor, and, although passive techniques usually are advocated, there is little doubt but that psychotherapy is what is being implied. Three or four articles propose, as does Hobbs (42), that in view of the limited number of psychiatrists and psychologists, it will be necessary to train teachers in modified psychotherapy principles.

The answer of the profession of psychiatry to this question of a short-

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age of trained therapists is that the treatment of milder neuroses should be turned over to the general practitioner. Eleven references occurred in the past year in which this was proposed, and in some of these references techniques to be used by the general practitioner were described. Typical are the discussions by Law (43), Ebaugh (44), and Overholser (45). Along this line, an interesting development was a two-week training course in psychotherapy for general practitioners, conducted at the University of Minnesota by a group of psychiatrists, and sponsored by the Commonwealth Fund. The course material is presented in a book by Witmer (46). Another answer of psychiatry appears to have been the flooding of the market with popular presentations of psychiatric concepts. Typical is Menninger's *You and Psychiatry* (47), which is illustrated by Leaf. Similar titles are *Understandable Psychiatry* (106), *Everyday Psychiatry* (107), and *Psychiatry for the Millions* (108)!

### Training of Psychotherapists

Another large group of articles deals with the training of psychologists as psychotherapists. Most important are the articles appearing under the editorship of Harrower (48), and contributed by a group of specialists taking part in a program sponsored by the Josiah Macy Foundation. Various prominent psychologists and psychiatrists participated, but implicit in the discussion of all was the need for psychologists to receive greater medical and/or psychoanalytic training if they are to go into therapy. Some of the contributors holding this view were Spitz, Brenman, Brosin, Kris, Hendrick, and Kubie (See 48).

Another symposium on this topic was conducted by the American Orthopsychiatric Association. At this meeting, considerable emphasis was given to the importance of the social case worker as a nonmedical therapist. Although ostensibly related to training, the discussion seemed largely oriented around a debate as to the place of the nonmedical therapist. Only Woltman (49) concedes that psychologists should conduct independent therapy, but he contends that no satisfactory training centers exist at present for psychologists, a point that a number of universities would undoubtedly debate. Luchins (50) has described a desirable training program for psychologists, basing his attitude somewhat on the opinions of the APA committee on training in clinical psychology. Articles by Blocksma (51) and Grummon & Gordon (52) describe the training at the University of Chicago, while Louttit (53) criticizes Rogers for training nonprofessional counselors in methods of psychotherapy.

### New Methodological Suggestions

It is probably difficult for therapists to think of further variations of psychotherapeutic techniques, but a number have been proposed during the past year. Rosenzweig (54) contends that the Thematic Apperception Test (TAT) has therapeutic implications. Fantel & Shneidman (55) propose a technique of starting a client with the Make a Picture Story Test

and then proceeding to a psychodrama based on the theme described in the picture. Lazarsfeld (56) turned the bibliotherapy idea around by asking patients to discuss the books which meant the most to them in childhood. Mueller & Kaney (57) went "all out" in the direction of combining techniques, using with one case narcosynthesis, hypnotherapy, active analysis, and case work therapy. Murray (60) proposes combining semantics with sociodrama. Another interesting development is the use of semantics in the treatment of schizophrenia (59). While Bixler (58) does not describe a new technique, he does discuss the value of limits in psychotherapy and recommends that they be more carefully employed.

### New Applications of Psychotherapeutic Techniques

Somewhat overlapping new methodological suggestions are the new applications of recognized psychotherapeutic techniques. Cotzin (61), Sarason (62), and Thorne (63) have used psychotherapy with good results with mental defectives. Faw (64), Gross (65), and Schwebel & Asch (66) have applied nondirective techniques to teaching situations. Six authors, as illustrated by Moreno (67), have done the same thing with psychodrama or sociodrama. A little on the side of the occult is Fodor's discussion (68) of the use of telepathy in psychoanalysis. Rogers (69) proposes the use of client-centered counseling as a means of solving interpersonal conflicts.

### Trend toward Briefer Psychotherapies

A slow but consistent trend toward briefer methods of psychotherapy is apparent. Alexander (70) is well known for his brief psychoanalysis. Six other authors defend the use of brief methods with a great deal of enthusiasm. However, two, Zilboorg (71) and Valenstein (72), hold out for the traditional full-term analysis and criticize short-term therapy as superficial.

### Use of Group Methods

A significant development has been the increase in the use of group therapy procedures. Ten authors have discussed factors which affect the use of group procedures. Loeser (73), for example, lists 12 factors, including homogeneous grouping, an optimum number of seven clients, and the use of fixed rather than continuous groups. Slavson (74) has published a new book on the subject; also, a round table discussion by Konopka and others (75) dealt with four phases of the application of group methods. A number of authors, as typified by Hobbs (76), have listed the advantages of group therapy over an individual approach. In four articles the use of group therapy with psychotics is discussed, all with favorable remarks, although its implementation with individual therapy is recommended. Articles by Coltharp (77) and Mann & Semrad (78) illustrate this use of the technique. A noteworthy study was conducted by Malone (79), who used social pressures in a community specially set up to produce thera-

peutic effects in a group of neuropsychiatric veterans. Special applications of group therapy have been proposed for use with groups of epileptics (80) and with patients' relatives (81). Several authors suggest the use of films, posters, sound recordings, and case histories as aids in group therapy (82, 83).

### Nondirective Therapy

One of the largest groups of articles dealing with psychotherapy to appear during the year was that relating to the nondirective approach. More than 40 were counted. A considerable number of these have already been described under previous headings. One group not described, however, deals with evaluations of the nondirective method. In a symposium in the *Journal of Clinical Psychology*, eight psychologists wrote frankly critical evaluations of the method. Ellis (84) proposed that most of Rogers' theoretical postulates (109) are not new but only restated concepts from traditional psychotherapy and/or psychoanalysis. Hathaway (85) discussed weaknesses in the criteria used in some of the studies cited in support of nondirective concepts. Hunt (86) contended that Rogers' postulate of therapy being adequate without diagnosis is an extremely dangerous one. Seidenfeld (87) believes there are many situations in which information-giving is quite beneficial. Thorne (88) feels that the nondirective method alone is satisfactory only for the treatment of minor personality problems. In another context, English (89) has proposed that the nondirective technique may be an obstacle to therapy by preventing the use of more directive techniques which the patient needs for rapid recovery.

### Hypnosis and Hypnotherapy

The final trend observed this year was a surprisingly large number of references to hypnosis as used in psychotherapy. Some of these articles are experimental but draw inferences as to the therapy process. Corn-Becker *et al.* (90) demonstrated the presence under hypnotic suggestion of "abstract conditioning" which, it is contended, is different from the generalization phenomenon described by Pavlov. Schafer (91) states, on the basis of an investigation using personality tests, that poor hypnotic subjects tend to be obsessive-compulsive persons with easily provoked aggressions and narcissistic emotional orientation. Sweetland (92) describes what he calls "hypnotic neurosis", i.e., under hypnotic suggestion subjects were able to raise their scores on depression and hypochondriasis on the MMPI. However, Varva (93) has shown that this can be done by unhypnotized subjects who are simply instructed to give responses which might be given by a hypochondriac. McNeil & Sparer (94) have produced two 15 inch phonograph records and demonstrated their usefulness in hypnotizing subjects. Gidro-Frank & Bowersbuch (95) used the phenomenon of hypnotic regression and found that three out of five subjects showed the infantile type of plantar response when stimulated on the sole of the foot.

Other articles relating to hypnosis discuss its use in psychotherapy. Wolberg (96, 97) has written a two-volume discussion on "medical hypnosis" which is definitive in character. Brenman & Knight (98) discuss limitations and uses of hypnosis and indicate that it should not be considered a separate method but rather an adjunct to other therapies. Conn (99) describes what he calls hypnosynthesis, contending that it is a dynamic, integrated experience that should be considered patient-centered. Weitz (100) feels that hypnosis is contraindicated in organic cases, with potential psychotics, and as an amusement technique but in other than these situations has much value. McDowell (101) describes a case in which various other methods of therapy applied over a period of some months had not been successful. Hypnotherapy was used with little success until an artificial conflict relating to sexual impotence and symbolic rejection was created. Following this treatment, almost immediate cure of all symptoms took place and continued for a follow-up period of a year.

## Summary

A number of significant trends have been observed in the literature on psychotherapy during the past year; these are indicated by the section headings in the present article. Most significant, however, is the volume of articles which appeared, there being more than 400 in all. Ten years ago there was not one-tenth of this number.

The most important general trend seems to be an attempt to fit psychotherapy into the general framework of psychological knowledge, particularly in interpreting it in terms of some sort of learning theory. Perhaps the second most important major trend is the extensive effort to test concepts of psychotherapy by research and experimental methods. The third major trend is the marked attention being paid to the question of who is qualified to practice psychotherapy, the question being raised especially with respect to psychologists and physicians in general practice. Finally, it is perhaps a trend that the individual schools of psychotherapy continue in enthusiastic fashion to extend their followings and to apply their techniques on as wide a scale as possible.

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# EDUCATIONAL PSYCHOLOGY[1]

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Educational psychology is presently changing its character and its outlook on life. The upheaval, resulting from many trends, is precipitated somewhat by criticisms voiced by the Policy and Planning Board of the American Psychological Association (1). National meetings, committees, and individual efforts are attempting to define the tasks of educational psychology and to revitalize research and theory in this field (2, 3, 4).

Having broken the bonds which confined it to Thorndikean studies of drill and individual differences, educational psychology has become increasingly concerned with the total socialization of the normal individual. Traditionally, educational psychology was oriented toward increasing the "efficiency" of formal education. But since learning takes place everywhere, all the social influences impinging on the child are educational forces. In view of this, the task of guiding development is seen to be exceedingly complex, and "efficiency" becomes almost indefinable. One committee (4) sees educational psychology as a "meeting place of all the studies concerned with the individual." The original concern with learning has been modified to place greater emphasis on meaningful problems, and the content of the field has been further swollen by the flood of findings in child study, mental hygiene, and, most recently, social psychology. Contributions from sociology and anthropology are also being woven into educational theory.

One price of this broader outlook has been an increasing overlap of educational psychology texts and courses with those in other fields. The books have grown huge, and the research extends over so wide and disconnected an expanse that no educational psychologist can be fully competent over the entire area. The Planning Board (1) notes that the field has declined in prestige. This decline may be the aftermath of the once-held expectation that the problems of education would yield to simple studies and theories. That hope having proved illusory, the claims for current work seem pallid by comparison. The present studies are limited, and integrative thinking is distinctly lacking. The solution of the crucial educational problems is beyond our immediate grasp, and there will be a frustrating delay before sounder theory matures; in the long run, the present plateau may prove to be a highly constructive period.

The first four sections of this review deal with studies seeking to clarify the nature and underlying dimensions of the learning process. The remaining portions consider work on specific practical problems.

[1] This review covers the period from January, 1948 to July, 1949.

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## Readiness

Readiness, a concept embracing aptitude, maturation, motivation, and experiential background, now holds a central place in educational theory. Two writers see readiness as determined largely by growth rather than experience. Courtis (5) reasons from growth curves and limited experiments that "growth, not teaching, is the primary factor which determines when and how much a child learns." Teachers are urged to remove pressures for progress and to permit children to grow at their own rates; this view is applied specifically to reading and spelling. Olson (6) takes a similar position. Over a decade, he measured children repeatedly on reading, dentition, intelligence, carpal ossification, etc. When reading age, dental age, and so on are plotted, a unique pattern of growth for each individual, common to both physical and behavioral variables, is said to appear. Remedial instruction, as in reading, is said to be wasted unless the child's "organismic age" exceeds his level in the subject in question. Because this challenges the usually accepted position, the conclusions can only be judged when full technical reports on the evidence are published.

Havighurst (7) points to developmental tasks as cultural determiners of readiness. In any society, certain learnings are expected of all members at a given age. The cultural pressures to acquire these skills and habits are a motivating force, and the tensions that follow failure in a developmental learning interfere with subsequent performance. Havighurst lists tasks for age levels from infancy to old age and suggests that the educational program be planned to help learners accomplish the tasks for which they are ready. He comments:

> There is no developmental task of children or adolescents which the school can completely ignore, for the reason that the tasks are so closely interrelated that difficulty in one task, which may show in the school, is often tied up with difficulty in another task for which the school has little responsibility. For instance, failure in academic work may be due to failure in some other developmental task.

Social background as a factor in readiness is considered by Davis and associates (8). They compared the performance of middle-class and lower-class children on intelligence test items. Lowers were less successful particularly on verbal items and certain informational items. Davis does not consider this evidence that the lowers are less ready to learn; he considers the tests unfair, in that they stress middle-class experiences and values. Tyler (9) draws from this and other studies the strong inference that lower-class children have untapped resources. He argues that the "biased" intelligence tests are effective predictors only because the schools also have a middle-class bias. The schools, he says, overstress verbal abilities, employ motivations that impress only the middle-class child, and base instruction on concepts meaningful only from the white-collar frame of reference. There is no evidence at present to demonstrate

that the schools can be changed to have more influence on lower-class children, but the challenge by Davis and Tyler should stimulate important experiments.

Whereas educational theory once stressed the necessity of waiting for nature to produce maturation before introducing topics, the above reports stress the role of experience in developing needs and understandings. Harlow (10) provides an experimental demonstration that readiness is learned. Children who learn one discrimination problem also learn much that shortens the learning time (facilitates "insight") on succeeding problems. The gain is presumably in learning sets, awareness of what cues to attend to, and other generalized habits. Davis' argument may be analyzed in these terms; essentially, he believes that lower-class children acquire different learning sets than middle-class children.

Moser (11) indicates that readiness may be present at an earlier age than is usually recognized. Teaching of fractions has been placed in later grades because of the belief that higher mental age is required for this topic. Moser taught fractions successfully to second-graders, by explaining the concepts in terms of experiences they had had. Not only did they learn to manipulate fractions correctly, but they continued to gain in skill even after the training had terminated, according to results of a retention test. A group of the same age, taught by a drill method, was unsuccessful. Brownell & Moser (12) also provide evidence that readiness in arithmetic depends on previous training. Readiness is evidently an interrelation between biological development, experience, and method of instruction.

### Social Class and Social Experience

Studies of the development of interests, character, and social skills are being extended to determine the influence of the child's social class on this learning. MacDonald, McGuire & Havighurst (13) obtained a diary report from preadolescents in a Chicago community. Activities outside the school hours showed marked differences between the social classes. The highest status group was more often found in scouting, family activities, and "cultural" activities such as homework, music lessons, and reading. The neighborhood club and evening play outside were conspicuously lower-class activities, and more of these children attended movies than was true of the highest status group. The lowest status group rarely participated in Boy Scouts or took music lessons. While most children participate in activities which teach the values of their social class, a minority in each class are learning, through leisure-time activities, attitudes which may help them to be mobile upwards.

Stendler (14) studied children's concepts and attitudes about social class. Children in grades 1, 4, 6, and 8 of a New England community were class-typed, tested, and interviewed in several ways. A striking difference was found between sociometric choices for in-school and out-of-school

situations. Within school, 35 to 41 per cent of first graders chose friends from their own class, but out-of-school the percentage was from 76 to 89. Similar differences were found for higher grades. The inference is drawn that the school has an important opportunity to promote friendly relations among children of different classes, a type of learning which is less likely in out-of-school contacts. A second technique was to present pictures of homes, men working, etc., and to request the children to tell whether they thought the picture related to wealth and high status or to poverty and low status. Thirdly, the children were asked to classify themselves and some of their schoolmates as to social status. Finally, a guess-who test was administered. Stendler concludes that awareness of social-class symbols develops gradually over a period of years and that in the early stages social status is not perceived separately from other desirable aspects of personality. Such "good" words as rich and nice go together. At the highest level of development, there is awareness that personality is an attribute of the individual, rather than a stereotyped attribute of class members. To some extent, however, stereotyped thinking was encouraged by the form of question Stendler employed.

Havighurst & Taba (15), with collaborators, studied the character of all 16-year-olds in Prairie City. In 27 chapters, they describe procedures, problems, findings, and inferences regarding the forging of adolescent character. The data came from extensive observation and testing of the pupils and the community. Integrated case studies demonstrated that character cannot be attributed to any single force. Correlation of reputation on moral traits with self-reported adjustment was 0.16; with social status, 0.32; IQ, 0.49; and achievement, 0.74. In this group, reputation appeared to be closely associated with the sort of conformity that is shown in good school achievement. Several personality types were identified; the same character may have different meanings in different personalities. If a person is basically submissive, he may show good conduct in the school, but shift to less desirable attitudes when he falls under another influence. A self-directing person will often accept moral principles and hold them regardless of his environment. Character formation is too complex for any single procedure such as indoctrination, religious education, or improvement of mental hygiene to be relied upon. The authors recommend that the school re-examine its practices, since the present system necessarily frustrates some pupils even if they adopt good conduct, and so pushes them to undesirable behavior. This is further illustrated by Robinson & Brady (16), who describe school activities and classroom situations through which children learn attitudes toward themselves and their peers.

### Emotional Factors in Learning

Learning theories were once developed independently of work on emotions and personality, but much current work is attempting to link these areas. Recent insights in psychotherapy are being applied to classroom

methods. In some cases, when a primary objective of teaching is the reorientation of attitude, converting the classroom into a group-therapy session has been found useful. Smith & McGrath (17) illustrate this in a class in child study for parents. The teacher employed "nondirective" procedures to encourage self-exploration of emotional attitudes, together with the usual development of concepts and principles. Favorable results were reported. Many writers have suggested that Rogers' methods (78) could be applied to the classroom, but these proposals seem premature. The suggestions outlined appear to be caricatures of proper nondirective counseling, even though in preliminary and inadequately controlled studies (18, 19) favorable evidence has been obtained. While use of the class hour for self-expression and self-consideration by the student may well be an important technique, a sound procedure will probably be developed only when some investigator applies concepts from therapy and derives a new technique for the classroom, rather than trying to transfer the precise techniques used by the counselor into the school.

Bills (20) finds that play therapy, given to retarded readers, produces marked and permanent gains in reading skill. Psychodrama has also been tried, and Grambs (21) reports on the theory and procedures underlying its use in instruction. Considerable research will be warranted to determine the proper place of therapeutic techniques as a supplement to conventional methods.

Disciplinary methods are discussed in terms of therapeutic concepts. Mowrer (22), who has been developing a theory of therapy which departs from those of Freud and Rogers (78), stresses the need for discipline in socialization, if the person is to be well adjusted. Surrogates for society, particularly the parent and teacher, must impose the values of society on growing children. This learning necessarily involves frustrating many of the child's wishes. Mowrer's recommendation is based on his experience with neurotics, who have not learned to "be good," he says, and so fear social disapproval. If conscience can be effectively instilled, it will promote later emotional adjustment. Imposed punishment instills a desire for social approval, so that the later adult will be sensitive to social pressures; but harsh, capricious, and unsympathetic discipline are certain to be unsuccessful. Mowrer's theory serves as a warning that excessively permissive methods may have as damaging consequences as excessive restriction. There is little clear evidence for Mowrer's theory as yet, and its emphasis on imposition is not in keeping with currently popular doctrines in education. The position is open to question if, by indoctrinating people with present social values, we make them less capable of thinking critically about social practices which need changing, and less capable of thinking independently. Perhaps the recommended technique makes for "adjusted" conformers. Kris (23) considers similar problems from a psychoanalytic standpoint and criticizes excessive indulgence. Indulgence reduces tension, but it stresses dependence and identification with the adult. "While indulgence and deprivation may both create unwanted effects,

both meet with some of the child's needs; the question is one of modality and timing."

In this connection, Grace (24) found that children react differently to reward and punishment. Using Peterson's rational learning problem (79), she confirmed the usual finding that praise produces greatest gains, and reproof produces more learning than neutral statements. The more suggestive portion of the study divides children into six groups according to their response to each condition. Each child had experienced each condition in turn. One group, for example, included those who did best with reproof, next best with praise, and least well with neutral comments. On Pintner's (80) and Rogers' (81) self-report tests of personality, Grace found suggestive group differences, although the groups were too small to yield significant differences. Children most stimulated by criticism were relatively submissive and conforming or were insecure and socially maladjusted. Extravert leaders seemed to respond best to praise. Although the study should be repeated with more cases, it demonstrates the need for considering what motivation is most suitable for particular individuals, and for determining why children of certain types respond to one control more than another.

Baldwin (25) studied the effect of control techniques in the home. The undemocratic, controlling home led to conformity with passivity and constriction in the preschool years; the democratic home led more to aggressiveness, fearlessness, and leadership. But, in contrast to the position often taken in discussing democracy, Baldwin found that different democratic atmospheres produced different effects. The general activity level of the home is highly significant; some democratic homes produce far more spontaneity and independence than others.

The child's relationship to authority is a central problem in socialization. In addition to the findings and theories reviewed above, the same problem appears in other guise in the studies of Havighurst and Taba (15), Symonds (82), and Frenkel-Brunswik (46) discussed elsewhere in this review. The problem is to find a way to produce citizens who are willing to control themselves according to accepted principles, without producing an unstable adaptation based on fear and dependence. Despite conflicts on points of doctrine, there is a large core of agreement among clinical and educational psychologists. The methods of the teacher and therapist have much in common. The teacher must identify goals and ensure reinforcement of desirable attitudes, and so also must the therapist (even the nondirective one). Differences regarding procedures, use of praise or blame, reliance on group processes, etc., are differences in preferred means, not ends.

## Learning as a Group Process

Whereas education was once conceived as interaction between pupil and subject matter, or between pupil, teacher, and subject matter, atten-

tion is now also being given to the learning that takes place through the interaction of group members. One aspect of interaction is its effect upon social learnings. Brady (26) points out that all classroom situations provide opportunities for children to develop skills required in group living. She denies that leadership is something one has or does not have and discusses methods by which the schools may develop the potentiality of all children for group membership and group leadership. Thelen (27), attempting to apply to education the principles of "group dynamics," takes a similar position. He examines the proper organization of classroom groups and states the hypothesis that "the size of the group should be the smallest group in which it is possible to have represented at a functional level all the socialization and achievement skills required for the particular learning activity at hand." He outlines pitfalls in organizing classroom group activity and suggests ways of avoiding them. This and other articles by those interested in group dynamics are full of fresh suggestions for school practice, but evidence on the propositions is almost entirely lacking.

An approach potentially useful for research in group learning situations is suggested by a group of college instructors (28). These writers develop a continuum to represent the extent to which the instructor in discussion imposes his points of view on the student. At one end of the continuum is the expository method; at the other end, the discussion is a group problem-solving activity. This scheme of analysis deals with the classroom discussion as an intellectual interchange, with relatively little attention to emotional factors.

## Research Methods in Educational Psychology

The hope that learning theory would lead in the near future to significant insights into education has dimmed considerably. Hilgard (29, pp. 357–58) surveyed the area and, after speaking of "the sterility of contemporary learning theories," concluded that while educators should look to learning studies for what help they can get, there are many educational problems which cannot be solved on the basis of present principles.

> An adequate research program in the applied psychology of learning would rest in part upon the findings in the experimental studies of learning, but it would consist in much more than the making of suggestions on the basis of general principles. There must finally be experimental testing in the school, or on the playground, or in the shop.

Anderson (30) and Brownell (31) similarly point out the futility of attempting to base school practice on any contemporary learning theory. The school situation involves interaction of many factors, and so does not follow the simple model of the laboratory experiment. Brownell stresses that educational psychology is not an attempt to apply psychological learning theory to education; rather, research must develop educational

principles out of studies of real school problems in their natural complex setting. McConnell (32, pp. 1–8) makes a similar point, criticizing those who once considered studies of nonsense learning a proper basis for recommendations to the school.

Research methodology is undergoing severe and disturbing examination. The classical two-group method, which is still used in many of the studies summarized below, has serious inadequacies. Such studies attempt to hold all factors constant save one; yet any experimental procedure will probably have its maximum effectiveness when all of the procedures in the classroom are simultaneously altered to harmonize with it. For example, a professor using the lecture method raises the question as to whether having two texts is more effective than just one. The proposed procedure is to teach parallel sections, carefully controlling the class presentation to say exactly the same things in both groups; but one of the real reasons for considering two texts is that by contrasting their viewpoints the class presentation can lead to deeper understanding. The "controlled" experiment deals with an artificial teaching situation. More complex designs are needed so that educational theory can recognize the interaction between materials of instruction, form of presentation, frequency and method of evaluation, and individual differences.

Solomon (33) finds that the two-group method ignores important sources of differences. He introduces a second control group, the third group allowing for the possibility that experience in the pretest alone may account for gains observed. In a small study, 10 children were taught a list of spelling words and given a pretest and post-test; 10 more children were used as a control group, taking both tests but no training; the third group took the training and posttest, but no pretest. The groups taking the pretest in this case were handicapped in learning the spelling; i.e., the conventional experimental design underestimated the effectiveness of the training. A four-group design is suggested to improve experiments wherein growth or the passage of events may alter the behavior being measured. Several studies in the present review illustrate other possible ways to improve design. Brownell & Moser (12) found somewhat different results when they replicated their experiment in three different schools. Anderson (32) compares different teaching methods, and by the Neyman-Johnson (87) technique found that whereas one method works best for some children, other methods are significantly superior for others in the same classes. Heidgerken (34) demonstrates effective use of the Latin Square design in education.

Bartlett (35) criticizes usual methods for measuring gains in skill learning. He urges that outcomes of training be measured, not only in terms of the total achievement or product, but also in terms of key elements in the performance. He supports this view with illustrations from wartime work on aviation in England. In any skill, the performer must make discriminations among certain sensory cues. Specially designed dis-

crimination tests for the key elements show which of them have been mastered. Key elements in the effector process can be studied only through observation. Even when two men have the same over-all achievement score, the perfection of their performance in terms of key elements may be quite different. Such differences in process are reflected in differences in retention, ability to perform under adverse conditions, and ability to transfer the skill to new materials. A similar recommendation for research is made by Thorndike (36). He urges measurement of method of performance, rather than altitude alone, in studies of ability.

Brownell (37) points out that inadequate criteria have distorted the findings of experimental comparisons of teaching methods. He echoes Bartlett's concern with the process of performance, rather than amount immediately recalled alone. Brownell suggests that in many experiments wherein two methods led to equal amounts of skill at the end of training, one may have presented the learning in such a way that it transferred more readily, resulting in savings of time in learning to respond to new situations. A third index of effectiveness which Brownell feels should be considered is the amount of the initial learning that is retained over a long interval.

The foregoing discussions indicate, and several of the current studies prove, that a good deal of previous educational research had led to false or inadequate conclusions. The reviewer is particularly impressed with the need to measure a learning experience in terms of its effect on readiness for subsequent learning (transfer value) and the importance of analyzing individual differences in response to treatments. Unfortunately, all of the suggestions point toward increasing the complexity of educational studies, and no one of the best present studies avoids all of the pitfalls identified above. In view of the limited usefulness of sheerly empirical generalizations, no matter how huge the samples on which they are based, we may look for greater emphasis on intensive studies, perhaps with small samples, which will yield understanding about changes in behavior rather than isolated facts about scores.

In this context, attention may be drawn to general discussions of test methodology. Buros' newest yearbook (38) includes reviews of instruments of all types, including 22 achievement batteries and numerous tests for particular school subjects, vocational guidance, and intellectual and personality development. Many of the reviews incorporate challenging viewpoints regarding evaluation. Mackenzie, for example, comments (83):

> Every possible hindrance to curriculum change should be removed. The use of standardized tests should be limited to individual pupils rather than total class groups, and their use should be optional on the part of teachers.

Another example is Davis' comment on a particular reading test (84):

> Rate of reading is an almost meaningless concept because an individual's rate of reading depends upon his immediate purpose and the difficulty of what he is

reading. In the ——— Tests, the directions for the rate of comprehension tests say, " . . . Read the story below very carefully so that you can answer questions about it." The testee has no idea what kind of questions . . . . Some testees will rush through and take a chance with the questions. Others will play it safe and digest what comes along . . . . Much more definite instructions would have to be provided in order to standardize the pupil's purposes.

An essay by Sims (39) stresses current weaknesses in evaluation, particularly the failure to test the meaningfulness of learnings.

### Investigations of Meaningful Learning

In contrast with the foregoing studies, which are concerned chiefly with general theory and methodology, numerous investigations have dealt directly with practical educational problems. These studies are largely independent and isolated comparisons of methods, with little development of theoretical implications; but a set of studies of meaningful learning in the classroom is an exception. Laboratory studies and the rise of field theory convinced most educators that meaningful learning was superior to rote learning, but there have been few studies of meaningful methods in the classroom. There is even some lingering doubt whether meaning is helpful, as shown in a study by Michael (40). He compared 15 classes which were taught algebra by one of two methods. Method A involved work with meaningful problems. Pupils were expected to discover generalizations without help or discussion of the algebraic principles, and then were given an opportunity to practice on further problems. In method B, the teacher stated a principle and then provided drill but did not explain why the principle worked. Method A yielded better attitudes toward algebra, and Method B yielded better scores on tests requiring application of generalizations. Michael believes that his results cast doubt on the advantage of meaningful procedures, but method A is so much a parody of a reasonable meaningful procedure that his results carry no weight.

Brownell & Moser (12) studied the process of subtraction, wherein earlier research had seemed to show that the "equal additions" (EA) technique is superior to "decomposition" (D). Most of this research had been done on older subjects, and the teaching had principally involved drill. Among third-graders, just beginning to subtract, some groups were taught each technique, and each technique was taught in some groups meaningfully, and in other groups as a rote process. Results were evaluated by various criteria including tests of retention and transfer to new types of problems. Interviews were used to determine how thoroughly pupils understood the methods. In general, the D method, taught meaningfully, gave better results than any of the other three approaches. With rote learning, children did not discover explanations for themselves, except for a group of children who previously had been taught arithmetic meaningfully. It was difficult to introduce rational teaching to classes

which had formerly learned mechanically. The meaningful method was consistently more successful with decomposition, but not with EA where the meanings could not be put over to the children.

Anderson (32) taught arithmetic to fourth-graders by meaning and drill methods. Many criterion measures were used including a transfer test, and the findings are complex. Anderson summarizes results for the tests of understanding as follows: drill procedures were superior for those pupils who ranked below the average in intelligence, especially if they also were above average in previous school attainment; the meaning methods were advantageous with pupils above average in general ability, especially if previous arithmetic achievement had been below average. In another study, Swenson (32) tested many hypotheses about transfer of training and retroactive inhibition, by teaching second-graders the addition facts under meaningful and drill conditions. The method stressing generalizations was much superior, even when some meaning was used in the drill groups. Under school conditions, retroactive inhibition was not troublesome, although the drill groups were more susceptible than the generalization groups.

The combination of the above impressive and extensive studies leaves little doubt that meaningful methods are of great value. But the studies did not prove that meaningful methods are always best; instead, they showed that previous research and theory, seeking a simple generalization, overlooked important complex relations.

### Other Studies of General Methods

Jones (41) determines whether individualized methods, which have been widely advocated, can be used practically. Teachers of several fourth-grade classes worked out individual programs for their pupils. When compared with conventional classes, these experimental classes showed consistently greater gains in reading, arithmetic, and spelling, the advantage being marked for children with IQ's below 90. The controls appear to have been loose, and it may be that the experimental teachers sacrificed some outcomes in order to devote more time to the skills tested. Even so, the evidence shows that individual work, suited to pupil needs, is practicable in typical schools.

Wartime use of training aids drew widespread attention, and led to strong claims for their effectiveness. Current research leads away from the uncritical belief that introducing films or apparatus always increases learning. Heidgerken (34) compared motion pictures, slidefilms, and motion pictures plus slidefilms in a course in Nursing Arts. None of the methods was more effective than teaching without the aids. Dudek & Glaser (42) produced one of the many careful studies of military trainers appearing in the aviation psychology program reports. A ground trainer for navigators simulated dead-reckoning conditions by means of a student booth equipped with aircraft instruments. The experiment was so

designed as to counterbalance differences between the trainer group and a group given similar problems by means of a blackboard. Analysis of variance was applied to measures of subsequent performance on criterion problems in actual flight. No significant advantage was held by the trainer group. Practical suggestions were made for improving the trainer and for utilizing the trainer and the classroom to supplement each other, rather than considering them as alternative methods. It is apparent that research must increasingly concentrate on defining the particular objectives and situations for which any technique is helpful, and on developing methods of utilizing learning aids effectively. The technical excellence of the Dudek-Glaser study makes it a model for needed studies in civilian education.

### Methods in Particular Fields

Problems of teaching in particular subjects receive continuing attention, with particular interest in reading, arithmetic, and speech. These studies are too varied to be concisely reviewed.

Numerous articles have dealt with intercultural or intergroup attitudes. The bulk of these publications are arguments for or descriptions of school programs. Williams' opinion regarding the inadequacy of research in the field of intergroup education applies to nearly all the publications of the current year (43, p. 8, p. 78).

> Over most of the field it remains true that the administrator, student, or interested citizen who wishes to gauge the comparative effectiveness of given programs or techniques can find little scientific evidence to guide him . . . . The number of relatively trivial studies which give static descriptions of unimportant phenomena concerning restricted and atypical populations is already more than sufficient. This is especially true of some types of attitude or opinion studies . . . .

Williams points out that certain assumptions have been made implicitly in all current programs and that research to justify these assumptions is absent or incomplete. Among the points of view he isolates are those which attempt to remedy only single factors (e.g., factual knowledge, economic conditions, or personality structure), overemphasis on either changing of attitudes or modifying tension-provoking social situations, the theory that contact alone promotes friendliness, and the assumption that special activities, such as interracial camps, will transfer into improved attitudes in all activities. Williams gives extensive suggestions for research. Sociometry as a technique for exploring intergroup attitudes is suggested in a monograph by Jennings (44), who illustrates how sociometric data are used to improve programs in schools. A preliminary study by Rothney & Hansen (45) evaluated a weekly radio broadcast designed to stimulate thinking about intergroup problems. The study illustrates the possibility of intensive field analysis of educational materials.

Frenkel-Brunswik (46) compared prejudiced and unprejudiced adolescents. According to the clinical evidence, the ethnocentric children tended

to think in terms of their own needs, admired power, were inflexible toward ambiguities, and saw the world as threatening. Their attitudes indicated ambivalence—a need to conform, but lack of security in relations with parents and teachers. This article suggests that intergroup attitudes are closely related to the mental health and social adjustment of the child. The suggestion is made that planned democratic participation in school and in family activities is an important means of effective socialization which might inhibit the spread of intergroup tensions.

Interest in foreign-language teaching was revived by wartime programs of intensive training for soldiers. The apparent merit of the intensive methods led to civilian applications, and to careful evaluation of the results. Agard & Dunkel (47) found that intensive work on speech did not lead to fluent speaking. Aural comprehension was learned to some degree but conventional programs produced greater reading proficiency than the newer method. A companion volume by Dunkel (48) examines the entire problem of language teaching, reviewing the educational psychology involved and pointing to unsettled issues. He suggests methods of designing standardized basic text materials which will conform to known psychological laws.

Among the interesting miscellaneous studies is Hahn's report (49) on children's speech. The quality of speech in a primary child varies, but is optimal when the speaker is thinking of the audience, the audience asks meaningful questions, and the teacher stays out of the discussion. Individual mannerisms such as garrulousness and poise appear even at this level. Hahn suggests that unfavorable habits become ingrained because the elementary school has no program to promote good speaking.

## Evaluation

Developments in evaluation have been concentrated particularly in the field of English. Two major reviews in this area have appeared. Travers (50) outlines the total range of objectives in English and points out that measurement of learning in English can rarely be accomplished solely by pencil-and-paper tests. He emphasizes the utility of procedures for identifying reading interests, and he reviews little-known work on children's ability to create poetry. He points out that ability to state an idea literally is a different writing skill from ability to express it aesthetically. The schools have given little attention to developing the latter. Studies of oral composition and aural comprehension are also urged. Harris (51) considers the evaluation of literary comprehension. He recognizes four operations in comprehension: translating the writer's symbols into more comprehensible ones; summarizing ideas or characterizations; inferring the tone, mood, and intent of the passage from more or less obscure cues; and relating technique and meaning, particularly in figurative style. Harris then develops recognition tests of seven aspects of comprehension, using 14 literary passages. A factor analysis of the resulting tests

indicates that, despite the logical differences among the various types of items, one general factor is sufficient to account for virtually all of the test variance. Even when materials as different as prose and poetry are read, there appears to be only one major factor. Evidently, even careful logical analysis of objectives may lead to a categorization of behaviors which does not correspond to the organization of individual differences, measured empirically. Harris (52) also reports a factor analysis of tests of language expression. The results, based on tests in two Indian tribes, are too complex to be summarized here; they indicate that a test which means one thing for one group may measure different elements in another group, because the students have come to the English language through different experiences.

The validity of the United States Armed Forces Institute (USAFI) English test was investigated by White (53). Unusual procedures included analysis of performance of classes on specific types of items in view of known objectives of the curricula they had followed, and anecdotal studies of individuals. Several instruments of use in the humanities are described in the report of the Cooperative Study in General Education (54, 55). An inventory of satisfactions found in reading fiction, and in other devices, was used to appraise students' goals in the humanities and as a core around which a program of general education could be constructed.

In a broad evaluation of nursing education, Nahm (56) examined beliefs of students. A high proportion of the students of nursing held authoritarian beliefs, although this varied markedly from school to school. Among other conclusions, she noted that a fairly high proportion seemed not to have a highly developed sense of social responsibility. Moreover, the students showed a general lack of psychological understanding. Nahm is adversely critical of the nursing school for its failure to develop more democratic attitudes in its students.

### Guidance Programs in the School

Although counseling and testing are dealt with in other chapters of this volume, attention here will be devoted to studies having specific relevance to educational planning. In the area of aptitudes and prognosis, several investigators reported correlations of the customary size between aptitude tests and school or college marks. In several studies, long lists of personality items or questions on study habits were tried, to determine which items discriminate achievers from underachievers. None of the resulting scoring keys promises substantial improvement of prediction on fresh samples. Of unusual interest is Rausch's report (57) on the importance of variability in prognosis. Freshmen at Indiana University were grouped according to the variability of their part-scores on placement tests and ratings. The members of the least variable group were consistently superior in achievement to more variable students who had the same average score.

Smith (58) studied the usefulness of spatial tests as school predictors. With Otis score held constant, his spatial score correlated 0.36 with marks in drawing; 0.66, with engineering drawing; 0.35, with art; and 0.34, with practical geometry. Wing (59) produced a significant new series of tests for musical aptitude. He criticizes Seashore's sensory type of test (85) and requires instead that the student judge, for example, the better of two harmonizations of a melody. For his seven tests, he found high validity, using the teacher's estimate of musical ability as the criterion. Even though the tests require previous musical training of a sort more common in England than America, they seem likely to be useful.

Implications for guidance and vocational training will emerge from Flanagan's refined technique of job analysis (60). Developed in wartime studies of aircrews, the "critical requirements" technique assumes that there are crucial characteristics that separate successful from the unsuccessful practitioners. Flanagan identifies the requirements for any task by observing workers and by obtaining anecdotal reports on failure or outstanding success in the task. These data are codified, and the resulting list of objectively-defined skills and habits is used for making a rating scale for evaluating training, for planning tests of aptitude, and for establishing educational objectives. The technique has been applied in one or more of these ways to airline pilots, Air Force officers, and scientists.

The value of guidance has been assessed in two studies. Lauck (61) compared adult delinquents and nondelinquents. Making allowance for inadequate controls and subjective methods, she concludes that vocational dissatisfaction is one concomitant of adult delinquency and that guidance in school reduces delinquency. Serious consideration must be given to the findings of Stone (62), who evaluated the University of Minnesota program for vocational orientation. Some freshmen took a course in career information, while others took elective courses. Those studying occupations acquired more knowledge about jobs than the controls, but both groups developed less favorable attitudes to many vital occupations. The trained group adjusted their vocational plans to more suitable levels, but their specific career choices were no better for them. Sounder career choices were made only by the group which was given individual counseling as well as the course. The group taking the informational course, with laboratory experiences but no counseling, actually shifted to less advisable choices. Considering that gains in other directions were probably made by the controls as a result of their other courses, the verdict is quite unfavorable to courses which provide vocational information but no counseling.

### Mentally Handicapped Children

Skeels & Harms (63) report on the intelligence quotients of children whose parents had low ability and who were placed in foster homes in infancy. The authors conclude that these children, during the early school

years, were somewhat superior to a random sample of the total population. The earlier and more dramatic report by Schmidt (77), who claimed to raise IQ's by remedial education, is placed under suspicion by Kirk (64), who attempted unsuccessfully to verify salient facts in the Schmidt report. Hill (65) performed an analysis similar to Schmidt's for another group of children given similar training and found no appreciable general gain in IQ. Shotwell (66) found that 42 per cent of institutionalized adolescent girls, with Binet IQ's between 60 and 69, could be rehabilitated and discharged. Shotwell recommends psychotherapy at this level. Evidently, changes in performance occur in some cases as a concomitant of improved adjustment; but there is little hope of increasing the ability of feeble-minded children generally. McIntosh (67) demonstrated that many mentally handicapped children have excellent potentiality for adjustment. Following 1,000 pupils who had been enrolled in a special trade school, he found that, as adults, one-fifth of them were as well off financially as the average industrial worker. Of those even with adult MA's around 8 years, three-fourths were self-supporting.

### Teacher Personnel

Barr (68) reviewed work on prediction of teacher efficiency. He points to serious limitations of previous research, but declares that in well-designed studies "multiple *R*'s in the 70's and 80's are not uncommon." The research he reviews does not provide an adequate guide for selecting teachers. He stresses the need for research on predicting success in particular duties and types of teaching, rather than in teaching-in-general.

Bowers (69) criticizes Barr's earlier studies in this area, but takes a position with which Barr's latest article evidently agrees: "There is not one kind of success in teaching, but hundreds. . . . The quest for an overall criterion of teaching success is an utterly futile one." In developing an aptitude test of his own, Bowers used subtests which measure personality, together with previous school marks. His test yielded correlations around 0.70 with performance in practice teaching, which he used as a partial criterion.

Barker (70) gathered unusually extensive data on teacher adjustment by means of an interview of the psychiatric type. The interview summary was rated by judges as to the degree of adjustment in various areas. The criterion rating of teaching efficiency had substantial correlations with work adjustment, and negligible correlations with adjustment in such areas as family and sex. Unfortunately, the only data reported are correlations corrected for attenuation, and since this process may involve unsound assumptions, the meaning of the findings is unclear. In view of the careful interviewing, however, the study appears to disprove the simple hypothesis that maladjusted persons make poor teachers. Gladstone (71) takes the position that maladjusted teachers need not create maladjusted children, disagreeing with an earlier review by Snyder (86). Gladstone

reasons that the effect of maladjustment depends upon the situation and the mechanism by which the teacher handles her conflicts. Evidently, assessment of teachers must be approached with hypotheses more complex than the correlational approach previously used.

### Other Summaries

Several reviews of topics in educational psychology have appeared. Recent issues of the *Review of Educational Research* deal with school plant, guidance and counseling, learning and the curriculum, subject fields, research and appraisal, psychological research in the armed forces, social framework of education, language and the arts, and teacher personnel. Chapters within the recent yearbooks of the National Society for the Study of Education review research on delinquency (72), audio-visual learning (73), and reading (74, 75). A book by Super (76) fills a long-felt need by bringing together all of the research on the validity of tests in vocational guidance.

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XUM

# COUNSELING METHODS: DIAGNOSTICS[1]

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The need for diagnosis in counseling has been a subject of discussion during the past year. Since 1942, nondirective counselors have stressed the needlessness and even the hazards of diagnostic procedures. In 1949 this point of view was presented in a comprehensive statement by Snygg & Combs (1). These authors, adopting what they entitle a "phenomonological psychology of individual behavior," state that the purpose of counseling is to assist the individual to perceive himself as he is, to accept this perception, and to behave accordingly. Diagnostic techniques, being imperfect, can only lead the counselor, who is another source of error, to arrive at erroneous conclusions about the client and thus retard rather than facilitate the process of self-understanding through which the client is progressing.

The contradictory point of view has been presented by Super (2):

> As a user of psychological tests and as a diagnostician of vocational aptitudes and interests the counselor has available information which may be of crucial importance to the client and of value to society. But neither the individual nor society may be aware of the availability and significance of that information; the client may never ask for it, and the counselor may never seek to obtain or to share it, if strictly nondirective procedures are used. One might ask whether it is ethical for a counselor to let a high school student work through his attitudes toward going to college and to make college plans without checking up on his mental equipment for going to college. Does a counselor who knows that a young man who is planning to enter a skilled trade actually has abilities and interests which might make him a scientist of considerable stature owe it to his client to make him aware of that fact? And does he owe it to society? It is not only attitudes which make for success and satisfaction: abilities, opportunities, and awareness also play a part. The counselor has an obligation beyond that of assisting the client to assume responsibility for his own action, although that seems to be the sole objective of counseling set up by the non-directive school. He also has responsibilities for the detection and optimum use of talent, and for helping some clients to achieve insights into themselves and into society which they might not develop in sufficient time if left to direct the entire course of counseling themselves.

The only reported experiment indirectly related to this problem is presented in a paper by Seeman (3), who examined the assumption made by the nondirectivists that clients, if given the opportunity, are capable of determining the kinds of information about themselves that will be useful in counseling and that they will select appropriate tests. A group of 170 clients seeking vocational counseling were interviewed by eight counselors

[1] This review covers the period from March, 1948 to July, 1949.
[2] Assisted by David L. Russell of the University of Minnesota.

who allowed the clients varying amounts of freedom of choice in selecting tests. Evaluation of questionnaires completed by the clients, of recorded typescripts of the interviews, and of tests selected led to the conclusion that "clients do take a free situation (test selection) and utilize it in terms of their own needs." Experimental evidence directly bearing upon the role of diagnostic information, whether client selected or counselor selected, within the counseling process, is still lacking.

In spite of these questions concerning the necessity for diagnosis in counseling, the research reports on diagnostic procedures constitute a dominant portion of the professional literature. Examination of the recent literature reveals several trends: (*a*) Individual workers are publishing comprehensive and frequently critical surveys and reviews of diagnostic tests; (*b*) attention given during the war to the development of brief, screening tests has continued; (*c*) continuing work is being done on the development of tests of special abilities; (*d*) professional aptitude tests are being developed in new fields; (*e*) implications for the interpretation of personality found in interest tests are being explored; (*f*) attempts are being made to develop new uses in counseling for the Minnesota Multiphasic Personality Inventory; (*g*) new uses of projective methods in counseling are being explored; and (*h*) lacunae in the research literature continue to reveal the lack of evidence concerning the validity of many otherwise carefully developed diagnostic tests.

## Surveys and Reviews of Techniques

The most useful single source of information about tests is Buros' new yearbook (4). It contains critical reviews of most of the tests used by counselors and many of these tests are reviewed by several authorities. Although the volume cannot be accepted as an infallible source of information, it provides an excellent starting place for one seeking to become acquainted with a test.

Another profitable source of information is Super's volume (2), which contains his evaluations of 42 tests most frequently used by counselors. This book is the first which has attempted to fit into the picture of diagnostic procedures the information acquired by military psychologists during World War II. Much of this information has not yet been assimilated in counseling today and Super hastens this assimilation by showing the relevancy of the information.

Super outlines three general methods of vocational diagnosis. Of these, one method involves the clinical evaluation of test data and utilizes three principal techniques: (*a*) conferences, (*b*) preparation of psychometric reports, and (*c*) discussion with the client. This method is favored over the methods of psychometric profiles (as used in the Minnesota Occupational Rating Scales and Occupational Profiles, the U. S. Employment Service General Aptitude Test Battery, and the Differential Aptitude Tests) and of psychometric indices (as used in the Kuder Preference Record and the U. S. Air Force "stanine" system).

Less comprehensive and more elementary reviews of diagnostic procedures are contained in the volumes by Froehlich & Benson (5) and by Wood & Haefner (6). The diagnostic use of tests at the college level is discussed succinctly by Darley (7) in a brochure prepared for the American Council on Education. The author attempted "to prepare a simple but definitive statement of the role that tests play in higher education."

The clinical use of achievement and aptitude tests is discussed by Horrocks (8) in a chapter in *An Introduction to Clinical Psychology*. This presents a bird's-eye view for the person not interested in a detailed account. Another less satisfactory source of cursory information is the *Encyclopedia of Vocational Guidance* (9).

The great number of tests in use today and the large amount of information available about many of them suggest that the usefulness of new comprehensive surveys may be limited and that what are more to be desired now are monographs devoted to single tests, such as those by Strong (57) and by Darley (58) on the Strong Vocational Interest Blank and that by Bell (59) on the Adjustment Inventory. Although test authors are tending to provide more comprehensive manuals, no monographs similar to these earlier ones have appeared recently.

### Abbreviated Intelligence Scales

Continuing the work started during World War II, Hunt and his colleagues (10, 11) have developed several brief individual intelligence tests based on the Kent E-G-Y Scales, Stanford-Binet Vocabulary, Army General Classification Test, and Wechsler-Bellevue Intelligence Scale. These tests have been developed primarily to assist in the screening of military recruits.

Otis & Chesler (12), however, have developed two forms of a short test of ability for use in the industrial and vocational counseling situation. Called the Classification Test for Industrial and Office Personnel, the test requires 10 to 15 min. and has correlations ranging from 0.62 to 0.83 with the A.C.E. (American Council on Education Psychological Examination), the Otis S-A Higher Form and the California Test of Mental Maturity. The odd-even reliability, corrected, is reported as 0.94.

### Tests of Special Aptitude

Further discussion of tests of specific abilities presented in preceding years has recently appeared. The Differential Aptitude Tests were criticized by Cottle (13) on the basis that the authors made no use of factor analysis in developing the tests, provided inadequate normative data, misunderstood the concept of test reliability, and presented inadequate validity data. The authors of the test (14) responded that Cottle's inferences about the norm group were in error and defended their failure to use factor analysis methods because factor analysis reveals different factors for different samples and provides scores which can be interpreted only by the use of concepts unfamiliar to many counselors. A supplement

to the test manual (15) presents validity data based on correlations between test scores and school grades. These correlations are statistically significant and in the expected directions, but it is doubtful if counselors will find that the correlations are of much assistance in the interpretation of the test scores.

Another battery of tests which was developed through methods of factor analysis is the Guilford-Zimmerman Aptitude Survey, described by the authors (16) as a series of seven tests with reliabilities, corrected, ranging from 0.88 to 0.92 and intercorrelations all below 0.27 with only one exception—that between spatial relations and spatial visualization being 0.48. These are the first to be described from a projected series of 20 tests of primary abilities. The tests are based upon the experience of the authors as military psychologists during World War II. As yet, no evidence of validity has been presented and the available norms are inadequate.

## Professional Aptitude Tests

Professional organizations during recent years have shown increasing interest in the development of aptitude tests appropriate for the purposes of selection and counseling in their particular fields.

Remmers & Gage (17) report a prediction study in pharmacy involving 3,200 freshmen entering approximately 40 schools of pharmacy in 1946. Multiple correlations between test scores and first semester grades ranged from 0.61 to 0.76, with the best combination of tests including the American Council on Education Psychological Examination, The Purdue Mathematics Training Test, the Purdue Physical Science Test, and the Cooperative English Test. After a detailed analysis of sex, school, and section of country for each test, the authors conclude that the efficiency of the battery is impaired by the high test intercorrelations and recommend the use of "factorially pure" tests.

The tests developed for the Division of Chemical Education, of The American Chemical Society, are described by Ashford (18) with respect to origins of the program, use of the tests, source of the norms, and plans for the future.

The tests developed for the American Society for Engineering Education by a committee headed by Vaughn have been subjected to criticism by Wren (19), who states that the tests were constructed without referring to the widely accepted objectives of engineering education. He does not approve of the criterion used in validating the tests (first term grades and drop-outs) and thinks that the validity correlations between test scores and these criteria (approximating 0.65) are too low.

Predictions of success in the law profession have been studied by Adams & Stuit (20), who used the 1946 revision of the Iowa Legal Aptitude Test. They conclude that the test is not a great deal better for predicting first-year grades than is undergraduate grade point average,

the correlations between test scores and grades ranging from 0.43 to 0.63, whereas undergraduate grade point average was correlated with first-year grades to the extent of 0.42. The multiple correlation between scores on the three best subtests (Reasoning, Relevancy, Information) and first-year grades was 0.59. In spite of the authors' pessimism, these tests appear promising.

Another previously developed aptitude test, the McAdory Art Test, has been revised and brought up to date by Anderson (21). Unfortunately, although she reports correlations of 0.98 and 0.96 between the original and the revised test, she reports nothing else concerning the validity of the scales.

### Expanding Use of Interest Tests

The use of interest tests in clinical psychology, particularly in vocational counseling, is discussed by Donahue (22), who makes no attempt to evaluate the available tests but describes methods of test interpretation. Diamond (23), making use of the 1940 U. S. Census data and the Los Angeles city directory for that year, demonstrates that the interpretation of interest profiles is dependent upon knowledge concerning the relative number and the characteristics of people in occupations. If percentile scores based upon norms for a general population are used, as was done with the Kuder Preference Record, the critical scores would have to be higher for those occupations involving a relatively small number of people than for the less selective occupations. This is a point frequently overlooked by counselors.

Super (24), in attempting to ascertain the nature of needed research, surveyed what is known about the relationship between vocational interests and vocational choice. His conclusions indicate that little is known about the development during adolescence of vocational interest and that longitudinal and experimental studies are needed. This type of research may well do more to provide counselors with useful techniques than has been done by research in any other direction.

Following a lead of Paterson (60), Longstaff (25), approaching the problem of test-taking attitude, studied the fakability of the Strong Vocational Interest Blank and the Kuder Preference Record. His subjects, after taking the tests under normal conditions, were asked to try simultaneously to lower scores on some scales and to raise scores on others. The results indicated that both tests were susceptible to faking and that the Kuder test could be more easily faked downward and the Strong test more easily faked upwards.

The work previously reported by Kendall (61) has more recently been followed up by Ostrom (26), who attempted to determine the relationship between the Occupational Level score of the Strong Vocational Interest Blank and scholastic success at three levels of ability. Although the author omits much relevant information from his report, his conclusions

appear to be supported by his data. Methods of analysis of variance revealed a "significant relationship between honor point ratio and both academic aptitude and Occupational Level in the Syracuse University freshmen sample."

A study of the stability of measured interests, using the Kuder Preference Record, has been reported by Fox (27). The blank was given to 58 boys and 76 girls in the ninth grade both before and after the group studied occupations, community opportunities, films, pamphlets, and guidance literature and received individual counseling. The absence of a control group detracts from the significance of the study but the relatively high correlations between pretests and post-tests, ranging from 0.42 to 0.85 for the boys and 0.54 to 0.85 for the girls, suggest a relatively stable organization of interests at this age level. The study should be repeated using more subjects, control groups, and additional methods of evaluating changes.

As an important personality variable involving the use of both interest and personality tests, "masculinity-femininity" has been the subject of three important papers. Heston (28) compared the masculinity-femininity scales of the Strong Vocational Interest Blank for Men, the Kuder Preference Record, the Minnesota Multiphasic Personality Inventory, and the DePauw Adjustment Inventory. Using small samples, 34 men and 45 women, he found that on each scale the distributions of scores obtained by men and women were significantly different and that the intercorrelations of the four scales ranged from 0.41 (Kuder and DePauw) to 0.73 (Kuder and Strong). By eliminating from the M-F scale of the Multiphasic those "interest items" present, the correlation between the Multiphasic scale and the DePauw scale was raised, demonstrating the influence of the type of item upon the intercorrelation.

Another study by Koch (29), based on a sample of 120 college student, male veterans revealed a correlation between the M-F scales of the Kuder and the Strong of 0.59, a somewhat lower correlation than that reported by Heston. Correlations between the two masculinity scales and the other scales of the Kuder test were also reported as being in the expected direction.

A brief report by Franck (30) has appeared describing his attempt to construct masculinity-femininity tests using a series of 36 incomplete drawings and a standardization group of 300 college students. Scores based on formal and content criteria significantly differentiated between the two sexes and had reliabilities of 0.84 or over. Sex differences were related to the use of space, lines, and content, the presence of closure, and the use of Freudian symbolism.

### Counseling Uses of the Minnesota Multiphasic Personality Inventory

This test has found wide use in hospitals, clinics, and offices of private practitioners. During the past year, it was used as part of the general

testing programs in at least five large universities and in several smaller colleges. Previous research has been directed toward the use of the test in a medical setting but recently an increasing number of studies with a counseling orientation have appeared.

Altus (31) attempted to develop a scale for the Multiphasic to differentiate between college achievers and nonachievers. Due to his using a small homogeneous group of subjects, an inadequate measure of ability, and a questionable criterion of achievement, his scale of 60 items is of little practical use to counselors but the results are a promising lead.

More successful has been Drake's work with the S-I-E (Social Introversion-Extroversion) scale for the Multiphasic. A recently published validity study (32) based on information obtained from 594 females counseled at the Wisconsin University Student Counseling Center led the authors to conclude: "The S-I-E scale significantly differentiates known groups of students varying in the degree of participation in recognized school activities and, therefore, is assumed to be a valid measure of social introversion-extroversion when activity participation is used as one criterion." The controls observed and the *t*-ratios, varying from 8 to 18, lend credence to this conclusion.

That the results of this study may be related to the socioeconomic status of the student is suggested by the studies of Hollingshead (33) and Gough (34, 35). The latter, using the Multiphasic, constructed a "Status" scale with a reported split-half reliability, corrected, of 0.74 and a test-retest reliability, using another sample, of 0.87. The Status scale had a correlation with the "Sims Score Card" (62) of 0.68 and with the "American Home Scale" (63) of 0.50. Correlations between the Status scale and other scales are reported to vary from −0.58 (with Social Introversion-Extroversion) to 0.33 (with K). Gough suggests that the people obtaining high status scores will tend to have an attitude of defensiveness and reserve in regard to personal affairs and problems, greater conventionality fewer somatic complaints, more satisfactory over-all adjustment, less tendency towards serious personality disorders, less insecurity, and less social introversion. This generalization must be accompanied by a reminder of the low correlations upon which it is based.

Increasing interest has been shown in determining the Multiphasic profiles characteristic of people in different occupations. Chyatte (36) tested a group of 50 male professional actors, 20 to 73 years old, and a matched group of nonactors. He found that the actors obtained significantly higher scores on scales for psychopathic deviate, masculinity-femininity, paranoia, schizophrenia, and hypomania and significantly lower scores on the scale for hysteria. The personality differences between occupational groups suggested by the Strong Vocational Interest Blank can apparently be measured meaningfully by other personality tests.

Other studies involving the Multiphasic done outside of the counseling context have relevancy for the counselor. The K scale developed by the original authors of the test and by Meehl (37) promises to be useful to

counselors using the test and to research psychologists studying test-taking attitudes. An attack on the same problem, using a different approach, is reported by Wiener (38), who developed subscales based on what he considered obvious and subtle items of the original D, Hy, Pd, Pa, and Ma scales. Later research may prove the discrepancies between obvious and subtle scores for the same scale to be clinically useful.

Hunt (39) has used the Multiphasic in still another way to study the influences of test-taking attitude. The profiles of 109 psychology students and 74 court martial prisoners were obtained under varying conditions: directions to respond truthfully, directions to conceal their personality abnormalities, and directions to malinger in a nondetectable manner. The deliberate deception was effective in distorting the profiles in the expected direction and the K scale was found to fail to supply adequate correction to these faked scores. An index based upon the F scale and the K scale was found to be more promising than the K score.

Counselors may find the coding system developed by Hathaway (40) for the Multiphasic profiles of value. More recently this system has been expanded by Welsh (41). It will help the counselor who has a great number, perhaps hundreds, of profiles in storage, but who, when examining that of a current case, is unable to remember the characteristics of previous cases with similar profiles. The coding system provides for the recording of both intensity and pattern, at the same time eliminating unnecessary details.

Another practical problem associated with the use of the Multiphasic was studied by Corsini (42), who conducted a time and motion study of hand scoring the test. During recent years several short-cut methods of scoring have been developed but Corsini's results indicate that the original method of hand scoring is no more difficult or time consuming than alternative methods.

No comprehensive studies of the validity of the Multiphasic are reported, with the exception of the standardization studies. Jennings (43) investigated the test as a means of screening out Army flying officers and combat crew members who were poor combat risks. He found a tendency for those people who later turned up as psychiatric casualties to obtain deviate profiles before assignment. Altus (44) studied the correlations between the schizophrenia scale of the Multiphasic and selected categories on the group Rorschach and found, among others, correlations of 0.31 and 0.41. The report allows no specific conclusions but suggests the need of additional research for determining the relationships between scores on these tests.

## Projective Techniques in Counseling

The use of projective techniques has been confined with few exceptions to medical settings. The reliability of analyzing responses to the Thematic Apperception Test has been reported by Mayman & Kutner

(45). Each of two judges, using several rating scales, analyzed 91 stories told by a total of 11 college students. Agreement between judges was determined. The small number of cases and the restricted number of judges prevent generalization, but in 89 per cent of the stories the two judges agreed as to the character with which the subject identified himself, and correlations for ratings of empathy were 0.91 and for ratings of emotional involvement 0.83.

Sims (46) in a suggestive but nonexperimental paper discusses the use of the essay examination as a projective device. Following Allport's lead (64), Sims approaches the essay examination as a personal document that well might reveal attitudes, values, and understanding rather than the acquisition of factual information. Another projective technique, the sentence completion test, was reviewed by Rohde (47). This test, seldom used by counselors, offers some promise.

A variety of other ideas about diagnosis not readily classified but of interest to the counselor have been published recently. Hollingshead (33) has emphasized the need for counselors to have information about the counselee's socioeconomic status and Warner (48) has suggested methods of obtaining this information. An important study by Friend & Haggard (49) reveals the important relationships between an individual's eventual work adjustment and his early experiences in the home, in the family, and on jobs. These authors conclude that the presence or absence of work adjustment "rests not in the mere possession of good work qualifications, but rather in the extent to which the client can marshal them; not in aptitudes, but in feelings, ideas, and attitudes. . . . One of the tightest links in our findings draws together the person's work and his personal adjustment."

Recognizing the diagnostic significance of the social history, Aldrich & Coffin (50) compared the social history and the Harrower-Erickson Multiple Choice Test (Rorschach) in terms of how well these two methods differentiated between men in the military service who were and who were not presenting psychiatric problems. The group Rorschach did not succeed in identifying potential psychotics but the social history was a fairly effective diagnostic tool.

Related to those procedures which study the individual in his role as a member of a group are the sociometric techniques which, as yet, have seldom been used by counselors. A review by Loomis & Pepinsky (51) suggests possible applications of these methods in counseling. The report by Jenkins (52) provides an example of how one of these methods was used in the Navy. One can readily think of situations in schools where the method could be applied.

Information supplied by Embree (53) assists in the interpretation of intelligence test scores as they are related to progress through college. Working with a group of 1,200 high school graduates who attended college, he found that the median IQ for college entrants as determined

during childhood, was 118; for those who received the B.A., 123; for those who received advanced degrees, 126; for honor graduates, 133; and for those elected to Phi Beta Kappa, 137. The range of IQ's for those receiving degrees was from 95 to 180. A somewhat similar study reported by Wrenn (54) supplies corresponding information for people receiving Ph.D.'s.

The problems of diagnostic categories have continued to receive attention. Pepinsky (55), modifying the categories suggested a few years ago by Bordin (65), found that the categories of "lack of assurance, lack of information, lack of skill, inter-personal self-conflict, and intra-personal self-conflict" helped in identifying origins of maladjustment, led to consistent diagnosis, and seemed relatively unique and distinctive. The categories of "dependence, cultural self-conflict and choice-anxiety" appeared less useful.

A new method of classifying personalities in terms of what a person does in meeting problems of every day life was proposed by Seashore & Jenssen (56), who also describe how they have used this method in counseling. Using concepts of problem-solving, goals, obstacles, and frustration and of a variety of personality dynamisms, they consider it a "time and motion study" method of approaching personality.

An area of research of importance to every counselor, but in which this reviewer could find no research publications, concerns the relationship between diagnosis and therapy. What diagnostic categories and techniques are most useful in selecting appropriate therapies? Because this question has remained unanswered, some counselors are willing to dispense with diagnosis. This question, and the parallel problem of validating existing diagnostic techniques, present the greatest research needs in counseling.

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# COUNSELING METHODS: THERAPY[1]

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The fluid state of the field demands that any review of counseling start by assessing current conceptions of its boundaries. In 1947, Wright & Darley (1), reviewing developments in counseling and guidance, found a definite trend toward a more dynamic approach to the individual. One result of this trend is to focus less attention upon counseling as an activity carried on in an education setting and as an activity concerned primarily with the making of educational or vocational decisions. During the last year the development of a broader conception of counseling continued at an accelerated pace. There is discernible a growing attempt to develop a conception of counseling which integrates it as a special subdivision of psychotherapy. Several articles reflect this tendency. In one, Bordin (2) argues that the distinction between personal and vocational problems is artificial and suggests two types of limitation, those of responsibility and those inherent in the method, which distinguish counseling from other forms of psychotherapy.

Since social caseworkers are going through a similar process of evolving a broader and more dynamic conception of their field, counselors may find further stimulus here for the clarification of their own thinking. The analogy between the two fields is particularly close because, just as counselors started by focusing on the everyday problems of educational and vocational choice and moved to a deeper concern with the significance of these problems in reflecting the personality development of the individual, so social caseworkers started with the problems of achieving financial independence and moved to more direct attention to personality development. In addition, both groups operate in a nonmedical setting. Of particular interest is Austin's analysis (3) of different types of psychotherapy and their differential relationship to casework and psychiatry. She seems to find casework emphasizing either purely supportive treatment or an intermediary between supportive and such more ambitious insight therapies as psychoanalysis. This point of view applied to counseling would suggest that counselors will be more concerned with individuals who are within the normal range of personal adjustment. Pepinsky's definition (4) is pertinent here. He defines clinical counseling "(*a*) as the diagnosis and treatment of minor (nonembedded, nonincapacitating), functional (nonorganic) maladjustments, and (*b*) as a relationship, primarily individual and face to face, between counselor and client."

The growing emphasis on personality objectives in counseling received strong support from the results of a ten year followup by Friend

[1] This review covers the period from January, 1948 to June, 1949.

& Haggard (5) of 80 cases counseled in the Vocational Advisory Service of the Family Society of Greater Boston. Their findings, based on rather extensive followup data, provided through both counseling and casework sources, point rather definitely to the deep roots in the personality development of the individual of his potentialities for satisfactory work adjustment.

Although the major emphasis in personality growth objectives of counseling is upon the client's emotional experiences in the counseling relationship, that of Berdie (6) gives us an opposite viewpoint. He stresses the role of auxilliary teacher for the counselor working in an educational setting. He describes the relationship as that of a responsible client working with a respected adult. This kind of relationship is suitable for enabling the client to make his goals more explicit and to acquire skills or deal with facts that cannot be treated effectively in the more formal classroom situation. There appears to be a kinship between these concepts and those presented by Thorne (7) in his discussions of the use of suggestion, persuasion, and advice.

## Theoretical Contributions

Writings in the field of counseling are still largely characterized by exploratory efforts at rationalizing the counseling process or some portion of it. There tends to be a large gap between such formulations and either their amenability to experimental test or, when an empirical test is possible, the execution of the verification process. In this section, we shall review the current contributions of this nature, leaving for consideration in connection with the appropriate studies those theoretical contributions about which there are actual data.

The nondirective formulations are still the center of attention. English (8) suggests certain limitations on nondirective methods wherein client anxieties or needs for information demand that the counselor exert types of control on the process that are not contemplated in a nondirective process. For example, he suggests that a client's anxieties and guilt feelings might be so strong that the counselor would refrain from or postpone reflecting her feelings. Thorne (9), a perennial suggester of limitations of nondirective counseling, discusses the use of pressure and coercion with individuals lacking adequate introjection of social controls. While there would be little disagreement that psychopathic personality traits indicate a poor prognosis for therapy, there still would remain the question of whether coercion is therapy. A different type of limitation is suggested by Bordin (10) when he points to limitations of nondirective concepts as descriptive devices. He calls attention to the fact that these formulations imply that variations in counseling method are best described by a single continuum, from directive to nondirective. Instead, he claims that these variations are multidimensional in nature and proposes what three of these dimensions might be: namely, the degree of responsibility assumed by the counselor for the course of the process, the

degree of the counselor's orientation and sensitivity to the client's attitudes, and the variation in the counselor's orientation toward dealing with the client's attitudes, ranging from intellectual argument to inducing emotional expression. Haas (11) presents a novel limitation of nondirective methods when he suggests that they do not go far enough in enabling the individual to take responsibility for his actions. He presents Moreno's psychodramatic techniques as a method of "self-directive" counseling where the client is enabled to translate his problems into action rather than words and, in that way, is enabled to participate deeply in the resolution of his difficulties and the restructuring of his emotional organization.

Kilby (12) presents an attempt to synthesize recent contributions from nondirective and other sources into a conception of vocational counseling methods which will take into account the client's other emotional needs as well as those arising from the need for making a decision.

Greco (13) feels that many times the client "works" himself around to some set of values or goals, and that this acts as a deterrent to the therapeutic relationship. He advocates a process of objective analysis of the alternatives to a decision and of the contingent factors which might govern the choice of one or another alternative. This attempt to "unbind" the therapeutic relationship from situational issues he calls the "contingency technique."

### Analysis of the Counseling Process

Within this broad area two major trends of research and thinking are apparent. One trend is toward an over-all analysis of the process; the other, toward the isolation for study of some particular aspect of the process. In the last year, the initial interview and the personality of the counselor have been isolated from the process as objects of study.

*The whole process.*—Even though a number of studies during the year focused on the whole process, most of them did not attempt to analyze specific relationships between counselor and client sequences, preferring to analyze either the counselor or the client's role or both through the process. One exception is the study of the role of client talk by Carnes & Robinson (14). They found positive relationships between the ratio of the amount of talk by the client to the total amount of talk in the interview and judgments of nature of the working relationship and the amount of "insight growth" of the client. Unfortunately, the meaning and validity of the latter two judgments are only available in an unpublished report. They found, also, relationships in the expected direction between the client talk ratio and the use of directive or nondirective methods. Finally, more client talk was found in units where clarification or tentative analysis were the primary counseling techniques than was found where interpretation or urging was the primary technique.

One of the major purposes of a study by Seeman (15) was to repeat Snyder's earlier study (16) of nondirective counseling to see how much

change in method there might be over a seven to nine year period. Although there was only one counselor common to them, Seeman found a remarkable degree of similarity between the two groups. He felt that the changes found were in the direction of greater nondirectiveness. Specifically, he found that the current counselors placed greater emphasis on restatement of content and reflection of feeling and less emphasis on simple acceptance, lead taking, and influencing the client's ideas. Although he finds a high degree of similarity in counselor methods from case to case, he also finds significant differences. Although Miller's study (17) is difficult to relate to Seeman's, one "feels" that there should be a relationship. She investigated the assumption that nondirective interviews would be characterized by predominantly neutral reactions on the part of the counselor. However, she found that these interviews were characterized by acceptance rather than neutrality, and one interview was even characterized by denial. Since Seeman finds a considerable reduction in simple acceptance, does this mean that the results are not based on sufficiently representative samples in one or both studies?

If one trend stands out in research studies emanating from Rogers and the group around him, it is toward the analysis of the client's behavior in the interview. The core concept here is the client's perception of himself and his environment. Raimy's pioneering study (18), quoted often but not published, was reported finally during the past year. In this study, verbatim records of 14 counseling cases were analyzed in terms of self-references in the client's verbalizations. Seven of the cases were classified as successful, five unsuccessful, and two questionable. These judgments were made by the counselor, the supervisor, and the experimenter on the basis of the case record plus a certain amount of unsystematic followup data. Since the judgments would seem to be based mostly on the case record, one cannot avoid the suspicion that the judges were using implicitly the change in the client's verbalization to designate success. The results of the analysis seem to demonstrate that in counseling that is judged successful, the client can be expected to progress from a preponderance of negative and ambivalent self-references to a preponderance of positive self-references. From Seeman's analysis (15), we obtain results which appear to modify or negate those of Raimy, but which are explainable on grounds that he, unlike Raimy, did not separate successful and unsuccessful cases in his analysis. Seeman, in analyzing all of the client's attitudes, whether directed toward himself or toward his environment, also found that at the beginning of counseling negative attitudes predominate, but although there is a change in the direction of more expression of positive attitudes, these do not become predominant at the end of the process. On further analysis, he finds that when the data are confined to attitudes expressed in the present tense, results similar to Raimy's are obtained.

The foregoing studies, and those of Hoffman (19), Stock (20), and Sheerer (21), bring into focus issues about the interpretation of the phenomena under observation. The latter three investigators used the

same set of cases as did Seeman. Hoffman analyzed client's reports of his behavior and derived from these an index of behavioral maturity. He found that successful cases increased on this index, whereas unsuccessful cases showed no change. Stock found positive correlations between client's attitudes toward self and others. Sheerer found that acceptance of the self and acceptance of others were correlated and increased with progress in counseling. In considering such findings, one cannot avoid being impressed by Hathaway's "hello-goodbye" analysis (22) of the social situation aspects of the counseling process. He points out that when a client comes to a counselor, there is some need to demonstrate that he is not wasting the counselor's time and has some good reason for seeing him. Therefore, he must tell how bad things are for him. When he begins to get ready to leave, social conventions demand that he tell the counselor how much help he has been to him. Until it is demonstrated that this pattern of change in client verbalization is correlated with "independent" observations of his behavior outside of the counseling hour, there can be no assurance that these verbalizations have more lasting significance than the response to the conventions involved in a specific social situation. This point would apply equally well whether the verbal behaviors are defined as attitudes toward self, attitudes toward others, or maturity of "reported" (by client) behavior. There is also the issue of "how different" are these supposedly different verbal behaviors of the client. When a client reports more mature behavior on his part, is he doing anything more than expressing a more positive attitude toward himself? When a client talks about his attitudes toward others, is not the usual intent that of trying to show something favorable or unfavorable about himself? If the answers are yes, then four studies of what was intended as four different aspects of the behavior of the same set of clients might really be four studies of essentially the same aspects of the clients' behavior. The intercorrelations of these four definitions of client behavior, reported by Raskin (23), might easily be considered at least a partial confirmation of this interpretation. Raskin combined data from the preceding studies to obtain the correlations and found a fairly high degree of correlation, particularly between self-attitudes and self-acceptance.

Less vulnerable to these criticisms is another study of the same set of cases by Haigh (24). He analyzed defensive behavior, defined as distortions in the client's perceptions, in response to a perception of threat to a value or concept which he holds as part of his concept of himself or his environment. When Haigh divided the sample into those showing an increase and those showing a decrease in defensive behavior during therapy, he found that the decrease group exhibited an increased awareness of defensive behavior, while the other groups showed the opposite trend. Similarly, for the decrease group, it was found that the relative frequency of reported defensive behavior increased as compared with a decrease in reported defensive behavior for the increase group.

It would be well here to call attention to the significance of the group

research project represented by the studies of Seeman (15), Hoffman (19), Stock (20), Sheerer (21), and Haigh (24) discussed above and that by Carr (25) which will be discussed later. This type of combined effort makes it possible to control more closely the sampling of clients and counselors for purposes of integrating results. One of the big problems of research in this field is our lack of knowledge sufficient to evaluate the comparability of samples of clients and counselors.

*Analysis of the initial interview.*—As Powdermaker (26) has suggested, the initial interview is a situation in which the patient or client tests out the counselor or therapist and establishes a relationship to him. Since in counseling situations emphasizing the discussion of educational or vocational decisions, the question of tests usually arises in the first interview, Bordin & Bixler's hypothesis (27) about the value of forcing client responsibility in the selection of tests has particular significance for the initial interview. Seeman, in setting out to test Bordin & Bixler's hypothesis, demonstrates the fruitfulness of focusing on this part of the counseling process. His first step was to analyze counselor's initial interview methods (28). This step enabled him to establish (*a*) the reliability of observer judgments of these methods, (*b*) that counselors vary in the amount of similarity to each other in their methods, and (*c*) that counselors exhibit a significant self consistency in methods. In a second stage of analysis, Seeman (29) studies the effects of Bordin & Bixler's suggested methods as compared with the traditional ones in terms of the number of tests selected and their relation to the types of issues that seem uppermost in the mind of the client. He found that approximately the same number of tests was selected by the self-selection method as by methods wherein the responsibility rested more heavily on the counselor. On the other hand, the number of tests selected under the self-selection procedure was more variable than under the alternative ones. However, it was not shown that this greater variability was reflected in any appreciable neglect of the informational needs of the client based on judgments of the types of questions on which he is seeking information and the appropriateness of the test for giving him that information. He was able, also, to explore the assumption that in forcing the client to deal with and accept considerable responsibility for the decision as to what types of information he was going to try to obtain from tests, the counselor was dealing with some of the critical issues in the individual's personality. Seeman found that those clients who showed more dependence and indecision in the self-selection process also exhibited more dependence and indecision in other phases of the interview. In the final study (30) of the series, he analyzed the client's perception of and reactions to the differences in methods. He found that clients experiencing the self-selection method are more likely than others to feel that the first interview was different than they expected. Clients are also able to designate with significant accuracy the self-selection methods used in their interviews. On the other hand, the client's favorableness to the interview experience was found to be unrelated to the counselor or method. Yet, when asked

directly to designate a specific rating to the interview, there was evidence that some nonchance factor other than the counselor's methods, perhaps his personality, may be a factor in that rating. When the client's anticipations of the nature of the future process were analyzed, it was found that clients of the counselor who most clearly used the self-selection method tended most frequently to anticipate participating in an active role in the next interview.

*Personality of counselor.*—Though psychoanalytic theories of therapy have given considerable attention to the personality of the therapist, counseling theory is only just beginning to pay some attention to this factor. In this context, the question is not what kind of personality makes a good therapist, but, more specifically, how the dynamics of the therapists' personality influences the roles he takes in the process and the types of interactions he has with the client. Though Wyatt (31) speaks in terms of the therapist working with definite neurotics, many of his observations about the ways in which the therapist's needs for self-gratification may interfere with the therapeutic process apply equally well to counseling therapy. Rogers (32) attempts to narrow the issue down to the philosophy of the counselor. He asserts that effective counseling is the resultant of that method of training which enables the prospective counselor to accept, and most completely is implement, an attitude of respect for the individual and his capacities to overcome his own difficulties. It is assumed that he does this most completely when he is able to get most fully inside the client's frame of reference.

Experimentation or other forms of analysis of the foregoing problem have been virtually nonexistent. Porter (43) has presented a proposed measure of counselor attitudes. This measure attempts to distinguish the following five attitudes: evaluative, interpretive, understanding, supportive, and probing. However, no attempts to use this measuring device are reported. The only other paper which approaches the problem at the empirical level is that by Hardy (33) on the relationship between dominance and degree of nondirectiveness. The latter characteristic was measured by asking counselors to propose responses to a recorded series of client statements reproduced by the experimenter. The personality tests used were the A-S Reaction Study, Inventory of Affective Tolerance, Terman-Miles Masculinity-Feminity Scale, Picture-Frustration Study, and a sentence completion test. Apart from the question of the appropriateness of the characteristics represented by the personality tests used, one wonders why the counselor's personality would be expected to play a part in such an impersonal situation. It probably is not surprising that the results were negative.

## The Effects of Counseling

The problem of obtaining outside of the counseling hour evidences of changes in behavior which can be attributed to the counseling process itself remains a formidable one. Travers (34), commenting on the poverty of such evidence, seems to attribute this condition to the unwillingness of

counselors to adopt the approach of the evaluator of educational methods in specifying objectives for his procedures that would then offer the basis for evaluation. Unfortunately, while the principle is valid, this argument apparently oversimplifies the problem by underestimating the tangled jungle of issues of general and personality theory, as well of counseling methodology, which lie in the path of an attempt at agreement on objectives. Faced with the responsibility for analyzing the effects of social casework, Hunt (35) took some steps along the path. He starts with the common sense assumption that the purpose of casework is to effect a change in the adjustive and adaptive habits, in the health, and in the environmental circumstances of clients. At this level of statement of the proposition few would disagree. They might not agree with the types of observations which are taken as the referents of these concepts. At least those who did agree would then have a common base for comparison of the relative effectiveness of their methods or programs. Hunt does demonstrate that agreement between caseworkers' judgments represented by a correlation of 0.70 can be obtained and that with training these correlations can be raised almost 0.10 points.

Meanwhile, rough and ready methods of estimating the effects of counseling continue to be applied as some counselors exhibit a laudable desire to demonstrate their effectiveness. For example, Brown (36) and Lipsett & Smith (37) followed up veterans who had been counseled in college Veterans Administration contract counseling centers. In both studies continuity in the training objectives chosen during counseling was used as evidence of attainment of desired effects of counseling. Both studies accentuate their roughness by the lack of concern for elementary controls. Neither considers what the expected percentage of continuity in training would be for a comparable noncounseled group. In one study (36) the questionnaire method was used and the analyses were based on replies from only a little more than half of the total population queried. That a representative sample did not reply seems likely from considering just one possible factor; namely, that the questionnaire did not reach the man because he changed his plans and moved elsewhere. Another questionnaire study reported by Stone & Simos (38) is based on a much higher percentage return, but differential returns between the experimental and control group place the conclusions in doubt. The investigators were attempting to compare counseling by personal interview and by letter. The study purports to demonstrate that there is virtually no difference in effect. It would be unfortunate if counselors were sufficiently convinced by this study to undertake counseling by correspondence. Before any hasty conclusions are reached, it should be emphasized that the counseling, as illustrated by the following quotation, apparently did not resemble very closely what is now thought of as clinical counseling:

> The counseling interview was normally one hour in length, and after discussions of work history, previous training, test results, and other factors related to the occupational adjustment of the individual, specific plans of action were worked out by the counselee (38, p. 404).

A final entry in the "rough and ready" division is Thorne's evaluation (39) of the amenability of mental defectives to therapy. Estimates of proportions showing improvement are reported by the investigator with virtually no evidence of concern for such refinements as how the criteria for improvement were defined, how much agreement there would be between independent judges, etc.

The remaining attempts at evaluation which appeared during the preceding year were more rigorous but did not offer any new solutions to the problem of criteria. A slightly different "wrinkle" in design is presented in the study by Kirchheimer, Axelrod & Hickerson (40) in which the difference between grade achievement before and after counseling was chosen as the criterion. An additional control was to select a noncounseled group. Both groups were then subdivided into those who changed their objectives and those who did not. The only group showing a significant change in achievement (positive direction) was the "counseled-change group." The "noncounseled-change group" approached significance (also positive). In view of the fact that the former group had the lowest precounseling level of achievement and that both groups were lower than the groups that did not change their objectives, one is sorry that controls on level of ability and high school achievement were not available despite the investigators' protestations that such controls were not necessary because of the high degree of selection exercised in admissions. The use of the Rorschach test, administered before and after counseling, as a criterion is exemplified by Carr's investigation (25). His study, based on an analysis of specific signs and on an expert's qualitative judgment, negates the previous study by Muench (41) by finding no evidence of changes in the Rorschach pattern after nondirective counseling. Consolation is found in the conclusion that the results indicate the limitations of using the Rorschach pattern as evidence to evaluate the outcomes of therapy and that a recent unpublished doctoral study found positive results by developing a new way of analyzing the test. The final study to be reported was made by Lipkin (42) in terms of the client's reactions to three questions asked at the close of the process. The questions were: (*a*) What seemed to be the story before you came in? (*b*) What seemed to go on during your visits here? and (*c*) How do things look to you now? Of 37 clients, 14 commented that the nondirective atmosphere seemed to force introspection, 12 claimed considerable gratification out of discovery of their own powers, and only two reported no assistance. In view of the fact that it was the counselor asking the questions, from the process the pertinence of Hathaway's "hello-goodbye" interpretation (22), mentioned earlier, needs no further elaboration.

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# STATISTICAL THEORY AND RESEARCH DESIGN*

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The scope and degree of coverage of the present review are necessarily limited. Primary emphasis is given papers clearly directed at the psychologist. New techniques and new results in statistical theory which seem to have a direct application to psychological research are also briefly reported. For actual application of the procedures here reviewed, the reader will, in most instances, have to refer to the original papers.

## Experimental Design and Analysis

The bulk of the current papers present new procedures of experimental design and analysis. A portion of these relate to design, but most with psychological applications develop new statistical tests or procedures of analysis and inference. The majority of the new statistical tests are highly specialized; but of these, a surprising number are well suited to the equally specialized problems of psychological research.

*Experimental design.*—Gagne, Foster & Crowley (1) studied the measurement of transfer of training to determine the advantages and limitations of the various quantitative measures of transfer which have been used. Six "relatively distinct" formulae were discussed in terms of theoretical and practical criteria. Two formulae (which turn out to be identical) were favored because they were independent of the score units, were not affected by variation in level of initial learning, and were more directly comparable from one experiment to another. Both give transfer as a proportion of total possible improvement. The best formula was:

$$T = \frac{(\text{Transfer Group Score}) - (\text{Control Group Score})}{(\text{Total Possible Score}) - (\text{Control Group Score})}.$$

Solomon (2) showed that added control groups were valuable in transfer of training and related experiments where pre- and post-tests were, used before and after the experimental training procedure. Ordinarily one experimental and one control group are used, as is shown in the first two columns of Table I. Solomon showed that a second control group, omitting only the pretest, is needed when the pretest procedure may produce an interaction effect with the training procedure. A third control group, omitting both the pretest and training procedure, is desirable if an uncontrolled, extra-experimental event may affect all groups, as fortuitous political events might affect a continuing study of social attitudes. The third control group makes pretest effects completely orthogonal to training effects; and this orthogonality permits independent evaluation

* This review covers the period from February, 1948 to June, 1949.

of the effects of: (*a*) training procedure effect, (*b*) pretest effect, and (*c*) the interaction of pretest and training.

Grant (3) pointed out a number of ways that the latin square principle could be used in the design and analysis of psychological experiments. The complete confounding of interactions and main effects in the two by

TABLE I

CONTROL GROUP DESIGN*

| Procedure | Experimental Group | Control Group I | Control Group II | Control Group III |
|---|---|---|---|---|
| Pretest | Yes | Yes | No | No |
| Experimental Training | Yes | No | Yes | No |
| Post-test | Yes | Yes | Yes | Yes |

* After Solomon (2).

two square was demonstrated in such a way as to make the experimenter aware of the difficulty of interpreting results from this design. An important use of the latin square was shown to render individual variation and progressive or ordinal effects (such as learning, fatigue, and transfer) independent of the effects of the experimental variable. Techniques for the analysis of replicated squares were given along with illustrations from learning, motivation, and motor skills experiments. As an example, in a reminiscence experiment in which four experimental procedures were to be used, it was proposed that each subject (S) learn one of the four lists of nonsense syllables under each experimental procedure. Then, if the number of S's were chosen to be a multiple of four, a number of greco-latin squares might be used to arrange the procedures. In particular, if 32 S's were used, they could be assigned to the 32 rows of eight greco-latin squares, with the experimental procedures assigned according to the latin letters, the lists assigned according to greek letters, and the order in which each S was to take the procedures assigned to columns; the separation of the degrees of freedom would then be as in Table II. This design should permit great precision in the evaluation of effects of the independent variable; and it provides information on list difficulty, practice effects, and individual differences as well. An example of an experiment on motivation and learning, which combined orthogonal design with latin square design, was also included in Grant's paper. Two additional applications of latin square design were outlined by Bugelski (4). Bugelski also pointed out that Grant's warning against squares of order $4n+2$ (which do not form greco-latin squares) should not prejudice psychologists against all squares of even order. He showed that the squares of even order permit more perfect balancing of sequences than do squares of odd order. Related to latin square analysis was Grant's statistical treatment of a frequently

occurring experimental design (5). An experimental (E) and a control procedure (C) are often used with each of two groups, one group going through the procedures in E–C order, the other group in the C–E order. If a total of $N$ S's are used, $N/2$ in each group, the variation should be

TABLE II

| Source of Variation | df |
|---|---|
| Experimental procedure (latin letters) | 3 |
| Variation in list difficulty (greek letters) | 3 |
| Order or practice effect (columns) | 3 |
| Individual differences between S's (rows) | 31 |
| Error estimate | 87 |
| Total variation | 127 |

analyzed according to the breakdown in Table III. An example of this type of analysis appeared in the paper. Failure to carry out a complete analysis according to the breakdown of Table III usually results in loss of considerable precision in the analysis.

TABLE III

| Source of Variation | df |
|---|---|
| Total variation | $2N-1$ |
| Experimental variable (E-C) | 1 |
| Sequence or Groups (E–C, C–E) | 1 |
| Ordinal position in sequences | 1 |
| Subjects within Sequences or Groups | $N-2$ |
| Error estimate (S's within sequences × Ordinal Position) | $N-2$ |

Kogan (6) discussed at length the analysis of variance when repeated measurements are available on S's who are divided into two or more groups. He showed how variation attributed to S's must be separated into "Inter-subject" and "Intra-subject" variation in order to obtain valid error es i-mates. The relation of the analysis of variance to correlation was stressed.

The important problem of determining sample size in the design of experiments has been discussed by Harris, Horvitz & Mood (7). Methods were outlined for situations in which specified degrees of accuracy were desired in the determination of: (*a*) confidence intervals, (*b*) significance of means, (*c*) differences between means, and (*d*) differences in randomized block experiments. Techniques of using previous data to provide "guessed estimates" of variance were presented, and the authors gave a very simple treatment of noncentral $t$.

The theory of experimental design has, at least temporarily, far outstripped psychological practice. For example, a completely orthogonal design wastes information when certain of the interactions are known in advance to be nonexistent. In psychological research where this may be true, the theory developed by Kempthorne (8) and Banerjee (9) on the weighing problem and by Brownlee, Kelly & Loraine (10) on fractional replication are now available. The work of Kempthorne & Federer (11, 12) on prime-power lattice designs may also prove useful but will remain largely inaccessible to the psychologist until he learns more about the algebra of groups.

*Analysis of variance.*—Three interesting papers concerning interpretation of the analysis of variance have appeared during the year. Johnson (13) contrasted the expected value of mean squares under conditions of (*a*) fixed and (*b*) variable interaction components. He showed that if the interaction components exhibit no sampling variation, the interaction mean square does not provide an accurate error estimate. If the interaction component is subject to sampling variation, the interaction mean square is the proper error term. Johnson's paper was closely related to Eisenhart's treatment of the assumptions in analysis of variance.[1] Finney (14) discussed the same problem in a more general fashion. He pointed out that the presence of interaction demands careful definition of the nature of a main effect, e.g., in an $r$ row by $c$ column design with $n$ observations per cell, the mean square for rows will have an expected value equal to the error variance plus $n$ times the interaction variance plus $n$ times $c$ times the row variance. He mentioned that this expression becomes further complicated when the number of observations varies from cell to cell. Here, interaction and main effects become inextricably bound together, and an elegant treatment becomes impossible. (Consistent estimates of interaction variance are usually inefficient.) Stevens (15) gave an excellent extended discussion of the computation of the analysis of variance from such a nonorthogonal experiment, but he did not attempt to cope with the problem of estimation. It is significant that Stevens urged experimenters to avoid such nonorthogonal designs if possible. Coombs (16) extended previous work of Peters (106) on the interpretation of analysis of variance in terms of correlation of row agents and column agents respectively, with the experimental variate. Interaction represents correlation between row and column agents. He pointed out the importance of securing representative values of the row and column agents in order to establish meaning for these correlations.

Computation of an analysis of variance often reveals a single outstanding mean among a group of means. The interpretation of such an extreme deviate is often difficult, but Nair's treatment (17) of the Studentized $u$, or extreme deviate, provided an objective test for rejection of outlying observations or the evaluation of an unusual mean. Another special

[1] EISENHART, C., *Biometrics*, **3**, 1–21 (1947).

problem arises when a large number of $F$'s are available, say from a factorial experiment. Among 20 such $F$'s, one is expected to be significant (5 per cent level) by chance, so that one or two chance $F$'s above the 5 per cent critical value are not unusual. Nair (18) developed the Studentized form of the extreme mean square test in analysis of variance and provided a table of the 5 per cent and 1 per cent points for the largest variance in a set of $k$ variances, each based on one df and all tested against a common error estimate based on $\nu$ df. The table covered values of $k$ from 1 to 10, and the values of $\nu$ were 10, 12, 15, 20, 30, 60, and $\infty$. Paulson (19) gave an approach for subdividing $K$ means into a superior and an inferior set after an analysis of variance test has proved that the means differ significantly. Both the normal and Studentized situation were considered.

Quenouille (20) has developed a fairly simple scheme for dealing with the nonorthogonality introduced into a design by missing values. He pointed out that his procedure is an extension of Bartlett's technique (107) and makes use of analysis of covariance. Another departure from original assumptions was dealt with by Aspin (21), who extended Welch's results (108), showing that absence of homogeneity of variance need not be serious if $n_1$ equals $n_2$ in a two sample experiment.

Madow (22) showed that the $F$ test is extremely sensitive to lack of control of the independent variable. In certain instances a 40 per cent drop in power takes place when slight variation is permitted among the replicates of the single cells of an experiment. This points up the desirability for precise control of the independent variable in analysis of variance designs. For example, if three differentially diagnosed groups of patients were selected for evaluation of improvement after four different therapies, slight variation in diagnostic criteria or in details of therapy could greatly reduce the power of $F$ tests used in analysis. Madow's discovery is of considerable importance to all experimenters, but is, perhaps, especially important to the psychologist because of the difficulties he has in controlling his variables. Madow suggested the desirability of seeking tests that were not disturbed by this bias, but the reviewer feels that such tests would probably lack power whether the variables were well or poorly controlled.

*Chi square and contingency tests.*—Yates (23) has developed a treatment for a special class of contingency table problems which are of the utmost importance to psychologists, particularly those in the clinical area. In his example, homework conditions were to be related to teachers' ratings of performance. Homework conditions were graded on a five point scale, and teachers rated the quality of preparation on a three point scale, so that a three by five contingency table resulted. The usual $\chi^2$ test for independence will detect any significant departure from proportionality within such a table, but the generality of the usual test is purchased at a cost of considerable power when a specific kind of departure from proportionality, such as linear regression, is involved. In the

example, it would be expected that superior teachers ratings would be earned by students who experienced good homework conditions. As an approximation the regression might be linear. Yates derived an exact test based on regression theory and also provided an estimate of the magnitude of the effect of each variable on the other. His example illustrated each step in the calculations. In the many instances, where available psychometry permits only rough quantification by means of rating methods, psychologists can profitably use Yates' methods.

Finney (24) has computed more complete tables for the exact treatment of two-by-two contingency problems. His tables are more convenient to use than Table VIII of Fisher & Yates[2] because direct entry is afforded. The new tables are restricted to cases where neither member of one pair of marginal totals exceeds 15. Another special treatment of contingency tables was provided by Bowker (25), who developed a $\chi^2$ test for departure from symmetry about the principal diagonal of a square contingency table. If $n_{ab}$ is the cell frequency in the $a$th row and $b$th column in an $m$ by $m$ table, and $\Sigma n$ is large:

$$\chi^2 = \sum_{i>j} \sum \frac{(n_{ij} - n_{ji})^2}{n_{ij} + n_{ji}},$$

with $m(m\text{-}1)/2$ degrees of freedom. For the two by two table Bowker's test becomes the same as that of McNemar[3] where:

$$\chi^2 = \frac{(|n_{12} - n_{21}| - 1)^2}{n_{12} + n_{21}}.$$

In McNemar's formula, the correction for continuity was suggested by Edwards (26). The correction for continuity should not be used in larger tables. Swineford (27) has computed tables based on McNemar's $\chi^2$ test, but she did not correct the formula for continuity so that her values are most useful for a large total $N$.

As another footnote to the historical Fisher-Pearson controversy on the application of the $\chi^2$ test to contingency tables, Mood (28) pointed out that by augmenting the test criterion to include estimates of the unknown marginal totals the (unchanged) Fisher tests "may be regarded as unconditional." A further controversial question arose in connection with the so-called $\chi^2$ smooth test. Here a regular $\chi^2$ test of independence is augmented by a $\chi^2$ based upon the pattern of the algebraic signs of the deviations from expectation in the independence test to produce the $\chi^2$ smooth test. The debatable point is the independence of the two $\chi^2$'s which are added together in the smooth test. Seal (29) was able to show that the tests are very nearly independent, but David (30) pointed out that stratification of the universe would probably invalidate the smooth test due to an increased number of moment restraints.

[2] FISHER, R. A., AND YATES, F., *Statistical Tables for Biological, Agricultural, and Medical Research*, 112 pp. (Oliver & Boyd, London, 1938)

[3] MCNEMAR, Q., *Psychometrika*, **12**, 153–57 (1947)

*Miscellaneous distributions and tests.*—Levene (31) has derived the distribution of $h$, the number of suit matches, in $n$ pairs of cards from $r$ different suits, where there are $y_i$ cards in the $i$th suit. The distribution is asymptotically normal with a mean, $\bar{h} = \Sigma y_i^2 - 2n/(4n-2)$, and a variance,

$$\sigma_h^2 = \frac{1}{4n^2}\left\{ (\Sigma y_i^2)(n+2) + (\Sigma y_i^2)^2\left(\frac{2n+5}{8n^2}\right) - (\Sigma y_i^3\left(\frac{n+2}{n}\right)\right\} - \frac{1}{2}.$$

Levine's test should be valuable to psychologists using matching techniques. For example, one might test the diagnostic properties of a complicated psychological test by matching protocols, or scores, with diagnostic group for a number of patients where the judge is told how many patients should appear in each diagnostic group.

Convenient diagrams for the two-tailed $F$ test have been published by Kirchen (32). The diagrams give $p$, the probability of an observed $F$ as great or greater than $F_0$ as a function of $F_0$, with degrees of freedom in two samples (of equal size) as the parameter.

Gottschalk (33) has developed a technique for splitting a symmetric bimodal curve into two component normal curves, such that the bimodal frequency function might be expressed as the sum or difference of the two component curves. This method treated only a restricted group of bimodal curves, but should be helpful in some cases of bimodality. A general solution for splitting frequency curves into component normal curves is not to be expected.

*Nonparametric tests and transformations.*—When assumptions of normality seem to be precarious, it is often possible either to use a test which does not assume normality or to transform the variate to some function for which the distribution will be more nearly normal. Available nonparametric techniques for the estimation of confidence intervals were surveyed in a very accessible paper by Noether (34). No new results or proofs were given, but the techniques were clearly explained. Walsh (35) has developed some very simple new tests for the significance of medians (and hence means) of continuous symmetric distributions which are valid under very general conditions. If the number of cases is less than 10, the tests are almost as powerful as corresponding $t$-tests. The raw scores are ordered, and simple functions of the ordered values, such as $\phi_0 = (x_1 + x_7)/2$, are found such that the probability that the median exceeds $\phi_0$ is less than 0.05. Tables of $\phi_0 = g(x_1, x_2, \cdots, x_n)$ are given for $n \leq 15$. Haldane (36) has found a simple exact test for birth order effects which would be valid for testing any sequential psychological processes such as learning, work decrement, dark adaptation, adaptation to an institution, etc. The test is related to the Wilcoxon, Mann-Whitney, and Festinger tests.[4] Haldane gave a differencing technique for obtaining probabilities and also developed the generating function and cumulants of the distribution. A

[4] FESTINGER, L., *Psychometrika*, **11**, 97–105 (1946); MANN, H. B., AND WHITNEY, D. R., *Ann. Math. Statistics*, **18**, 50–60 (1947)

very limited table was computed which may serve as a starting point for further calculations.

Mueller (37) discussed numerical transformations in psychology at some length and provided a good list of references. Mueller showed how transformations aid in testing theory, in describing data, and in providing valid statistical tests. No mention was made of the probit transformation or Fisher's $z$ transformation for $r$, and the arcsine transformation got only a brief sentence. Transformations for Poisson, binomial, and negative binomial distributions were developed by Anscombe (38). Both theory and examples were discussed. Another transformation was proposed by Haldane (39), who used inverse probability to show that for small frequencies $\sqrt[3]{f_0}$ is approximately normally distributed, but Pearson (40) pointed out that exact treatment in terms of the incomplete beta function is possible. He preferred the direct attack rather than Haldane's "obviously attractive" solution.

*Power functions of statistical tests.*—Psychologists do not, at present, make much use of power functions, but it seems probable that they will in the future, because the power function expresses the probability that a given statistical test will detect various departures from the null hypothesis. Power functions are available for the common statistical tests and are becoming available for various specialized tests. Bateman (41) has calculated the power function for the longest run, when single and double dependence in a sequence of observations are to be detected as departures from randomness. He showed that the number-of-groups criterion is more powerful than the runs criterion in detecting single dependence as in simple Markoff chaining, but that runs and grouping criteria are almost equally powerful in detecting double dependence. In detecting more complex dependence the runs may be more powerful. Since runs and grouping of successes may be used as statistical tests in psychological research[5] this finding is of some importance. Preliminary treatments of the power of two-by-two contingency tests have been reported by Patnaik (42) and Pearson & Merrington (43), who provided tables and graphs. Walsh (44) showed that the loss of information occurring when the $t$ test is used in instances where the population variance is known is approximately equal to $K_\alpha^2 n/2(n-1)$, where $K_\alpha$ is the standardized normal deviate exceeded with probability, $\alpha$, the rejection level being used, and $n$, the number of cases in the sample.

*Sampling.*—An empirical study by Hochstim & Smith (45) showed that area sampling was more representative than quota control sampling, and that under the experimental conditions, callbacks could be safely eliminated. Another approach to eliminating callbacks was reported by Politz & Simmons (46). Each respondent was asked to estimate his time not at home, and his replies were weighted by the reciprocal of his "proportion of time at home." A theoretical basis for the technique was de-

[5] GRANT, D. A., *Psychol. Bull.*, **43**, 272–82 (1946); **44**, 276–79 (1947)

veloped, and empirical tests indicated that unbiased estimates could be obtained in practice. If this technique stands up under extended tests, it will prove a great asset in area sampling.

### Psychometric Methods

*Psychophysical techniques.*— Finney & Stevens (47) have published tables for the calculation of working probits and weights in probit analysis. They cover "finer units of the argument" for values of Y from 1.00 to 9.00. This extends the range of available tables.[6] The superiority of the probit method to previously available techniques for fitting psychometric functions makes these tables welcome, although previous tables will be accurate enough for most psychological applications. Berkson (48) has compared probit analysis with his minimum $X^2$ technique and notes the similarity of the two solutions when the distribution functions are symmetric.

Dixon & Mood (49) have derived the formulae for the "threshold," $\sigma$, and confidence limits for the "threshold" of a new sampling technique for analysing sensitivity data. The technique is directly applicable to determination of group or individual psychophysical thresholds and has the advantage of concentrating testing in the region of the threshold. Briefly, the experimental technique is (*a*) to test with the stimulus; (*b*) if the first stimulus is supraliminal, test at the next lower level; if the first stimulus is subliminal, test at the next higher level; (*c*) repeat until the threshold is crossed; and (*d*) start a new series. As is so often the case, the technique is not new, but the mathematical rationale for the analysis is definitely new and represents an advance in psychophysical methodology.

*Scale analysis.*—Guttman's scale analysis has come in for considerable discussion and improvement, chiefly by Edwards. Edwards (50) pointed out inconsistencies in Guttman's technique of establishing "cutting points." He showed that adhering rigidly to a procedure based on the assumption of perfect reproducibility would result in an objective technique of cutting between ranks and would maximize reproducibility. He also demonstrated that a large number of extreme attitudes among the S's would spuriously inflate the reproducibility. Edwards & Kilpatrick (51, 52) summarized the Thurstone, Likert, and Guttman scaling procedures and went on to show how combining all three fills out one of the most troublesome lacunae in Guttman's procedure, namely, the selection of original items. Furthermore, the procedure has been demonstrated to produce scales of a dozen or more items which are not "mere restatements of one another" and which fit the criteria for scalability with reproducibilities often greater than 0.90. The Thurstone technique of equal-appearing intervals assures a suitable range of items and eliminates some of the

[6] Finney, D. J., *Probit Analysis: A statistical treatment of the sigmoid response curve*, 256 pp. (Cambridge Univ. Press, London, 1947).

neutral members of the scale, which tend to lower reproducibility. Item analysis applied to the chosen items scored by Likert's method will then eliminate certain items which do not discriminate, but could not be rejected on the basis of Thurstone's technique. Scale analysis of the remaining items combines convenient scoring with assessment of reproducibility. Another variant of scale analysis was proposed by Dodd (53), who pointed out that a zero entry in each fourfold table for correlation between items is a necessary and sufficient condition for scalability. He proposed a $\chi^2$ test, corrected for continuity, to test for this. (Cochran's $\chi_m^2$ technique[7] would provide a better test of the hypothesis than that proposed by Dodd.) Dodd did not provide an over-all test for scalability, but adhered to an item by item approach. He went on to discuss prediction of opinions from their subclasses in terms of probability, correlation, geometry, and symbolic logic.

*New approaches to scaling.*—Loevinger (54) described and criticized the techniques of scale analysis and of factor analysis and pointed out that her own technique of homogeneous tests is in some respects superior to either scale analysis or factor analysis. Her index, $H_t = (V_x - V_{\text{het.}})/(V_{\text{hom.}} - V_{\text{het.}})$, where $V_x$ = test variance, $V_{\text{het.}}$ = variance of a hypothetical completely heterogeneous test with equal distribution of item popularities, and $V_{\text{hom.}}$ = variance of a hypothetical completely homogeneous test with the same distribution of item popularities, is maximal when the items are homogeneous. She also defines $H_{ij}$, the ratio of $\phi_{ij}$ to $\phi_{\max.}$ for an inter-item correlation, and $H_{ii}$, the proportion of correct discriminations by the item minus the proportion of incorrect discriminations. Her indices are efficient estimates of homogeneity but have not as yet been theoretically related to each other or to other common indices (except $H_{ij}$). There is little doubt that Loevinger's techniques will accomplish many of the aims of scale analysis, and they deserve an extensive trial. Data are not yet available to determine which technique will be superior in a given application.

Coombs (55) developed some hypotheses regarding the analysis of qualitative variables, such as personality traits. He showed that a test could, theoretically, be made to yield six meaningful scores or indices of trait averages, group averages, variance between individuals, variance within individuals, etc. In a further discussion (56) Coombs provided a rationale for the estimation of each of these scores. Another technique of scaling, which does not imply an elaborate theoretical basis, has been developed by McCormick (57), who weighted the data of a contingency table according to the percentages within the table. Distances were established as inversely related to the change in percentages from cell to cell, and a rough multiple correlation technique results. McCormick illustrated his procedure by an example relating type of crime to the vocation of the

[7] COCHRAN, W. G., *Iowa State College J. Sci.*, **16**, 421–36 (1942).

prisoner and his family. A still less elaborate scaling technique, a way of ordering $n$ items assigned to $k$ rank categories by $m$ judges or raters, was described by Schuyler (58). The advantages of Schuyler's procedure are that a ranking can be achieved even if (*a*) some of the judges fail to rank some of the items and (*b*) these judges have frequent tied ranks. No attempt was made to evaluate the reliability of the resulting ordering.

### Correlation and Multivariate Analysis

All recent contributions to correlation and multivariate analysis may conveniently be considered in this section. Thus included will be zero order linear correlation, reliability, multiple regression, multivariate tests, and factor analysis.

*Zero order correlation.*—Moran (59) has investigated the nature of the estimate of the universe $\hat{\rho}$ provided by the Spearman rank difference correlation coefficient, $\rho$. Assuming a normal universe he found that $\rho$ provides a biased and inconsistent estimate not only of $r$, the Pearson product moment coefficient, but also of $\hat{\rho}$. He presented tables of the expected value of $\rho$ and of $r_s$ for values of $\hat{\rho}$ of 0.1, 0.2, . . . , 0.9, and $n = 5$, 10, 20, and, $\infty$. The limit value is provided only for $E(\rho)$. Moran pointed out that it is extremely difficult to calculate the standard error of $\rho$ and that Kendall's $\tau$ is much superior as an index of rank correlation. Olds (60) has continued his investigation of the sampling variability of rank difference correlation and has calculated tables of the upper and lower bounds for the 95 per cent region of $\Sigma d^2$ for $11 \leqq n \leqq 30$. The 5 per cent significant $\rho$ can thus be determined. Lev (61) has proved that the point biserial $r$ is the maximum likelihood estimate of the universe value of the product moment correlation coefficient, $r$, and that:

$$t = r_{p-bis} \frac{\sqrt{n-2}}{\sqrt{1 - r^2_{p-bis}}}$$

is distributed as noncentral $t$ where

$$\delta = r\sqrt{n}/\sqrt{1 - r^2}$$

*Reliability and validity.*—Grubbs (62) has discussed the estimation of product variability and the precision of the measuring instruments in the acceptance inspection context. He suggested means of distinguishing between inaccuracies of the measuring instruments and actual product variability. The problem is that same as that faced by Cronbach[8] and others in the context of psychological testing, and the suggested solution is similar. Hsü (63) called for new definitions of validity. He proposed to invert the usual interpretation. He suggested: first, predict the test from several criteria; secondly, correct all coefficients for attenuation; and finally,

[8] Cronbach, L. J., *Psychometrika*, **12**, 1–16 (1947).

combine the corrected coefficients in a multiple $R$ which would be a sort of supervalidity coefficient, measuring the totality of meaning of the test score. No practical usefulness was claimed, but even on a theoretical plane the procedure would seem to be rather atmospheric. At a more practical level, a generalized expression for the reliability coefficient has been derived by Horst (64) to fit the situation when the number of measures varies from individual to individual in the group. He found the solution to be

$$r_{tt} = 1 - \frac{\Sigma \frac{\sigma_i^2}{n_i - 1}}{N\sigma_m^2},$$

where $N$ = the number of S's, $n_i$ = the number of measures for $S_i$, $\sigma_i$ = the S.D. of ratings for $S_i$, and $\sigma_m$ = the S.D. for the means of the $N$ S's. A simple computational procedure was illustrated, and the Spearman-Brown prophecy formula and the Kuder-Richardson formula 21 were derived as special cases of the above. Horst (65) has also shown how each test in a battery may be shortened so as to permit unit weighting of each test score in the multiple regression equation. The procedure saves testing time at a relatively small cost in lowered vality.

*Multiple regression.*—Ferguson (66) showed that maximum discrimination will be obtained when a test produces a rectangular distribution of scores, which lead him to define a coefficient of test discrimination:

$$\delta = \frac{n^2 - \Sigma f_i^2}{n^2 - \frac{n^2}{k+1}},$$

where, $n$ = the number of individuals, $f_i$ = the number of individuals with a score, $i$, and $k$ = the number of items. The coefficient, $\delta$, is the number of difference relationships among the scores on the test divided by the number of difference relationships for a comparable test producing a rectangular distribution of scores. Gengerelli (67) suggested a simplified method of approximating multiple regression coefficients. The $\beta$ coefficients were computed sequentially, and the procedure was terminated as soon as the additional coefficients seemed to add insignificantly to the correlation. The procedure does not yield a least squares solution, and the $\beta$'s and $R$ will vary, depending upon the order of computation. The method seems to be less useful than the Wherry-Doolittle test-selection method or the standard Doolittle method.

*Multivariate tests.*—Several contributions to multivariate analysis have appeared this year and, interestingly, all the writers used psychological data for illustrations. Analysis of variance provides excellent tests for differences between groups when one variate is involved, but for much psychological work, particularly for work in diagnosis and prediction, groups

may differ in two or more variables; and a test is sought for detecting systematic differences between groups, making use of all available scores. This is the problem of multivariate analysis, and much has been accomplished in this area which can be of direct use to psychologists.

Rao applied his analysis of dispersion technique (68) to the differences between differentially diagnosed neurotic group of officers in a study with Slater (69). Thirteen scores were available on each officer, and these scores were classified into three groups: (*a*) inadequacy, (*b*) instability, and (*c*) shyness. The scores were used to set up matrices based upon the sums of squares and cross products. The ratio of the determinants of the *between groups* and *within groups* matrices were used to calculate Bartlett's $\chi^2$ to detect over-all differences between groups. Further analysis was then made on each group, and an $F$ test of the "generalized distance" between group, which is essentially the same as Hotelling's generalized Student test (109) or Fisher's test (110) of the significance of the discriminant function, was described. After establishing the significance of the difference between groups, Rao & Slater proceeded to discover whether or not the difference was along a single dimension. This was done by factoring the product matrix and testing the significance of the latent roots (after the first) by means of $\chi^2$. Finally, techniques for calculating linear discriminant scores were described. The procedures of Rao & Slater seem directly applicable, particularly in clinical psychology. Multivariate analysis provides a powerful and general means of intergroup analysis.

Cochran & Bliss (70) showed how "covariance variates," the counterpart of the psychologist's "suppressor variables," can increase the significance of the discriminant function, even though these variates are not correlated with group differences. A numerical example was provided in terms of differences in training evaluated in groups which were equated for IQ. Votaw (71) has derived a test for "compound symmetry" which should be extremely useful in psychological test construction. Compound symmetry is defined as obtaining in the situation where each of a group of tests has an equal mean and variance, the covariances between all tests are equal, and the covariances between all members of this group and one or more criteria are equal. This situation should hold among equivalent forms of a test and any group of outside criteria, so that the general test provides a stringent measure of the equivalence of a number of forms of a test.

*Factor analysis.*—The area of factor analysis continued to be active, but theoretical and methodological advances were not numerous. Dwyer & MacPhail (72) developed an elementary differential calculus of matrices. Starting with the accepted definitions of the derivative of a matrix with respect to a scalar and the derivative of a scalar with respect to a matrix, other matrix derivatives were defined by extension of these definitions. Applications of the differential calculus of matrices to correla-

tion and linear regression, orthogonal regression, canonical correlation, discriminant functions, and factor analysis were outlined. The techniques provided should prove useful in establishing brief but complete proofs for a great number of correlation problems.

A number of criticisms of factor analysis have appeared recently. Jeffress (73) pointed out that the restrictive character of the assumption of no variation in factor loading from individual to individual is widely ignored in factor analysis, a point which was mentioned by Loevinger (54). Insofar as this can be answered, it has been been by Thurstone,[9] but the repetition of such basic criticism seems to the reviewer to be a desirable thing. Bentley (74) takes Guilford (113) to task for overenthusiasm in claiming that "fundamental scientific gains" were achieved by factor analysis in the USAAF selection program.

An analytic discussion of the effects of chance errors on factor analysis was contributed by Saunders (75). Unreliability was shown to produce errors in factor loadings proportional to the unreliability of the measures. Multivariate sampling errors will throw normally unique error loadings into the common factor space to produce spurious results. Errors of rotation could not be satisfactorily evaluated in the absence of an analytic or semi-analytic technique of determining the rotation to simple structure. Objective rotation methods are, of course, available, but it was pointed out that they do not provide a probable error for the loadings. Minimizing the sum of the squares of the tangents of angles between the test vectors and the hyperplanes of the structure was proposed as an analytic criterion for satisfactory rotation. A theoretically interesting contribution to factor analysis was made by Rosner (76), who demonstrated that it is possible to obtain an algebraic solution for the communalities by appealing to the Cayley-Hamilton theorem that a square matrix satisfies its characteristic equation. Substituting the matrix written with $d_i$ as diagonal entries into the characteristic equation results in a matrix of polynomials in the $d_i$'s. Since each polynomial must equal zero, selecting $n$ independent equations permits solution for the $d_i$'s. Too much computation is required for this method to be practical, but it is of theoretical value.

New factor analysis techniques were proposed by Guilford & Michael (77) and Gengerelli (78), while Thurstone (79) was able to relate two older procedures. Guilford & Michael proposed to estimate factor scores by means of one heavily loaded test in conjunction with one or more univocal suppression tests to eliminate impurities. Solutions in terms of maximizing the desired factor, minimizing the undesired factor loadings, materially reducing the latter without reducing the former, and selecting a specified ratio of desired to undesired variance, were derived. Also a graphic portrayal of amount of desired and undesired loading as a function of the

[9] THURSTONE, L. L., *Psychol. Bull.*, **37**, 189–236 (1940).

suppression weight was developed; this might be used in selecting optimal suppression weights. Gengerelli's method of analyzing psychological functions was to compute $\beta_1$ and $\beta_2$ from the frequency distribution of scores, and, setting these equal to the equations for $\beta_1$, and $\beta_2$ for the distribution of the binomial, $(p+q)^n$, to solve these equations for $p$ and $n$. The number of factors involved in the function is estimated by $n$. Gengerelli claimed that the fact that such an estimate was very unreliable" . . . is perhaps a virtue in the method . . . since it serves to impress the worker with the importance of sampling. . . . " This sort of procedure is hard to evaluate; it will be difficult to compare it with other factoring techniques. Thurstone was able to show that his "multiple group" method of analysis is essentially the same as Holzinger's "simple method." If the simple "method" is applied in repeated cycles until the residuals vanish, the methods are the same. Of course, the method is no longer Holzinger's "simple method," (111) either.

Estimating oblique factors still presents problems. Thomson (80) showed how oblique factors may be estimated by computing reciprocals of $r$ by $r$ order matrices, where $r$ is the number of factors. This usually results in a considerable reduction in the amount of work required by Ledermann's solution (112). Harris & Knoell (81) described the differences between Thurstone's simple axis solution and Holzinger's primary axis solution of the oblique structure. Holzinger's transformation matrix was shown to be the transpose of the normalized inverse of Thurstone's transformation matrix. Transforming from one oblique solution to another led Harris (82) to discover a technique by which a single rotation to primary structure may be obtained. Good agreement was obtained with previous techniques. Fair objectivity and agreement seemed also to hold for more complex new problems.

### Coding and Computational Techniques

*Coding.*—Toops (83) described his addend method of coding both from the theoretical and the practical standpoint. A large number of examples were given, and it was shown how the addends yield code numbers which identify uniquely the ultimate breakdown groups within the data. The edge-marking system of coding cards was described by Thurstone (84) and 13 uses were outlined. Lester (85) pointed out that edge-marking can be combined with rim-marking to obtain an even more effective classification and coding system. He described the use of rim-marked cards during the war in the analysis of aircraft accidents.

*Computation.*—Kossack (86), Adkins (87), and Tucker (88) described procedures for calculating zero order $r$'s once the sums were available. Adkins' and Tucker's methods are similar to Kossack's but use only one matrix and provide cross checks. Ellis & Riopelle (89) have devised a new technique for obtaining $\Sigma X$, $\Sigma X^2$, and $\Sigma XY$ for zero order $r$'s. Their procedure requires a sorter, tabulator, card counting collator, and a sum-

mary gang-punch. Although it uses many cards, it is economical of operator time and is especially good for higher moments. Hamilton (90) and Goheen & Kavruck (91) described new methods for computing $r_{tet}$. Hamilton's technique involves a new monogram, and the Goheen-Kavruck contribution involves a worksheet to be used in connection with the Hayes computing diagrams.

Mosteller & Tukey (92) have published a long paper on the use of binomial probability paper. They showed how the paper may be used for: summing deviations, sign tests, obtaining the o–c curve for sign tests, confidence limits for the median, analysis of variance, planning binomial experimental designs, testing for homogeneity, bioassay, etc. References and tables to aid in certain computations were included.

## General Treatments

A number of bibliographies and other general treatments of statistics, particularly psychological statistics, which have appeared during the year will be cited in concluding the present survey of recent work.

*Bibliographies.*—Swineford & Holzinger (93) published a selected list of papers on statistics and test construction which appeared from March, 1947 to March, 1948. The emphasis was on test analysis, and most papers were briefly annotated. Buros' "Statistical Methodology Index," Nos. 11, 12, and 13 (94, 95, 96) containing 739 references appeared in the *Journal of the American Statistical Association*. The usual completeness of these lists was sacrificed because of space demands, and, unfortunately, the publication of the Index seems now to have been terminated.

*Books.*—Three excellent statistical books by psychologists have appeared during the year. They have the common property of making considerable demands on the student, but they emphasize different statistical topics. Lewis (97) has a general treatment of functional relationships and curve fitting in addition to extensive treatment of statistical distribution functions and their use in the psychological context. McNemar (98) has written a concise but rather complete text for a first year course in psychological statistics. The strongest of points his book are the treatment of linear correlation and the five chapters on small sample theory. Johnson's book (99) seems to be the most complete and useful treatment of experimental design and analysis that has been written for psychologists and educators. It should be a handy reference for researchers in most of the biological and social sciences. Johnson has filled his book with detailed examples of the analysis of psychological and educational data. Special chapters on non-normal assumptions and on sampling theory and practice are certain to be helpful to the research worker in the field.

Other statistical books, not directly psychological in scope, include an elementary statistics text by Wilks (100), a book on sampling inspection by Freemen *et al.* (101), a treatise on rank correlation by Kendall (102), and an edition of the *Kelley Statistical Tables* (103).

Churchman (104) has written an extremely interesting book on the general theory of experimental inference. It injects a broader (and in some instances, controversial) philosophical note into the discussion of experimental design and is to be recommended to advanced students. Also concerned with some of the broader issues of probability analysis is an interesting paper by Anscombe (105) which challenges many of the basic precepts of modern statistics. (The challenges were ably taken up by distinguished members of his audience in the pungent discussion which apparently follows so many papers before the Royal Society.) This paper and the discussion might profitably be read by advanced students of psychology who are interested in the general theory of inductive inference. It serves as a reminder that all systems of experimental inference are based upon postulates, and that in 1949 there is still legitimate disagreement about these postulates.

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# PROBLEM SOLVING AND SYMBOLIC PROCESSES[1]

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Psychology in the middle of the twentieth century finds it hard to draw a line between problem solving and learning. Prevailing usage seems to list an experiment under the heading of problem solving rather than learning when the situation is rather complicated and the activities involved correspondingly variegated, and when the experiment is focussed on the processes by which the solution is first attained rather than the improvement of repeated performances of the solution. The solution of complex problems, especially by human beings, often includes symbolic processes, such as the formation and use of verbal and numerical concepts, hypotheses, and plans of action; hence the acquisition and use of such symbolic material is particularly pertinent to our topic. Furthermore, because intelligence and other abilities are in large part abilities to solve problems, our topic overlaps the topic of individual differences in a special way, and we shall see that some of the most enlightening contributions are coming from the analysis of individual differences in problem solving.

Progress in this area of psychology during the past year or so has not been impressive, research being more often directed toward simpler and presumably less elusive processes. For this reason Hilgard (1, p. 345), concluding his recent *Theories of Learning* with his own point of view, emphasized ideation and suggested a startling inversion of Morgan's canon: "An organism capable of ideational problem solving may also use ideas in learning situations in which they would be theoretically unnecessary."

As to general treatments of our topic, Hilgard has a section on problem solving, given over primarily to the work of Maier; Heidbreder (2) has surveyed methods of studying human thinking; Underwood (3) has included a chapter of experiments on thinking; and Postman & Egan (4) offer something rather new in experimental psychology, a chapter on general principles of judgment.

## Experimental Analysis of Problem Solving Processes

One factor in problem solving that has been emphasized in recent years is Maier's "direction," a new set or steer developed during solution of the problem. Weaver & Madden (5) have repeated, with minor modifications, Maier's 1930 experiment on "direction," the problem being to construct supports and pendulums to make chalk marks on the floor. The

[1] This paper covers the period from March, 1948 to July, 1949.

subjects were 52 college students. One group was given only the problem, another, the problem and hints about various parts of the solution, another, the problem and a hint about the direction to look for a solution, and the fourth group received the problem, the parts, and the direction.

Only ten subjects solved the problem, and five of these, as would be expected, were in the fourth group. The group aided by an explanation of the parts of the solution produced four solutions, while those helped by the direction produced only one. The experimenters conclude from the objective results and from their observations during the experiment that Maier has overemphasized the importance of new directions obtained by reasoning in the problem situation. They emphasize previously learned directions, habits of search, exploration, or investigation, but they state "that this experiment does not seem to provide an objective means by which habitual and new directions can be distinguished." Another limitation on this rather inconclusive experiment is the small number of subjects in each group.

In an ingenious and thorough experiment which was directed toward the operation of set, especially as it limits variability of behavior, Guetzkow (6), a student of Maier, seems to have separated susceptibility to set from ability to overcome set. Large numbers of college students worked two problems under group conditions. In the Luchins series of volume measurement problems with imaginary jars of water, the first five problems can only be solved by a complex indirect method, the next two, either by the same indirect method or by a more direct one, and the last, only by the direct method. Use of the practiced indirect method on the two ambiguous problems is taken as a measure of susceptibility to set. Failure on the last one is taken as a measure of inability to overcome a set once established. By these measures, Guetzkow finds that men and women are equally susceptible to set, but men are significantly more able to overcome set.

Guetzkow's other problem was Maier's well-known two string problem adapted for group use. The instructions ask for many types of solution, three of which are assumed, on the basis of previous work, to be reproductive or habitual, while a fourth, the pendulum solution, is assumed to indicate productive thought. An index of stereotypy or inclination toward habitual solutions is obtained by dividing the number of solutions by the number of different types of solution. There was no sex difference in stereotypy, but men gave more nonhabitual or productive solutions than women. This finding may be rephrased to parallel the results from the first problem: there was no sex difference in susceptibility to set, but men were more able to overcome set than were women. Parenthetically, Guetzkow, like Weaver & Madden (5), found that a hint about direction, swinging a key on a string like a pendulum, was ineffective.

In respect to Guetzkow's measures of set the two problems seem to be quite specific. Those who clung to the habitual method on the volume

problem were only slightly more stereotyped on the two string problem than those who shifted their approach. Similarly, those who failed the last of the volume problems offered only a few less pendulum solutions to the two string problem than those who solved it. Neither difference was statistically significant. Therefore, in additional comparisions Guetzkow used small extreme groups: 22 who were especially susceptible to set on both problems for contrast with 25 nonsusceptibles, and 25 who solved the last of the volume problems and also gave pendulum solutions to the string problem for contrast with 47 who failed to do either. The nonsusceptibles were able to give the critical solutions much more often than the susceptibles. This seems to show that the separation of susceptibility to set from ability to surmount set is not at all distinct.

These extreme groups were then compared in respect to scores on tests which have been analyzed factorially and on a 12-item reasoning test constructed by Guetzkow of the kind of problems used by Duncker, Maier, and Wertheimer. In respect to susceptibility to set, the nonsusceptibles scored higher on the Thurstone Flags, the Gottschaldt Figures, and the 12-item reasoning test, but lower on the American Council on Education Test (ACE), presumably because in the former three tests much shifting is required, while on the ACE, continuance of a set is facilitative. In respect to ability to surmount set, solvers were not differentiated from failures by a composite of three fluency tests, nor by Thurstone's letter series tests intended to measure inductive reasoning. The solvers did better, however, on the Gottschaldt Figures and on a test of finding human and animal figures hidden in a landscape. These last two tests "demand freeing the desired figure from the configuration in which it occurs."

It is unfortunate that Guetzkow's research has not been published in a form that will circulate widely. In addition to incidental results not mentioned here, it shows how experimental arrangements can establish a set and how that set in turn can limit variation of problem solving attempts. It shows how this susceptibility to set and the ability to overcome it can be separately recorded by a group procedure, even though individual differences in the two aspects of set are not uncorrelated. Comparisons between these results and scores on the tests help to clarify both the factorial composition of the tests and the requirements for success in solving the problems.

Marquart (7), also a student of Maier, took his technique for developing abnormal fixations in the rat and adapted it to the human animal. The stimuli are presented on two small doors, as in the jumping apparatus, but each door is opened by a metal handle that can deliver an electric shock to the subject's hand. The stimuli are of various shapes and sizes, arranged in various combinations. The subject is told to find "a pattern running throughout the situation." A no-solution problem is presented first, the subject being arbitrarily punished regardless of his choice. Then

a simple position problem is presented, the correct card always being on the left.

The most interesting results were obtained with Group I, a group of 42 college students. They were arbitrarily punished 75 per cent of the time for 50 trials; then, without comment, the door on the left was made the correct one. Learning a position habit, i.e., always using the handle on the left, would of course be very easy if not preceded by the frustration of the no-solution problem. As it turned out, 11 subjects required more than 120 trials to master this simple problem, and two failed entirely. A graph of the distribution of learning scores indicates that the arbitrary punishment divided these students into two groups, one composed of those who mastered the problem in less than 100 trials, the other composed of those who required more than 120. No one scored between 100 and 120. Marquart takes this bimodality as evidence for Maier's distinction between motivation and frustration in instigating behavior. On the basis of the learning scores this group could be clearly divided into the "normal learners" and the "slow learners." (Additional calculations by the reviewer add some support to the presumption of bimodality.)

One of the effects of frustration, of prime importance for problem solving in general, is illuminated by Marquart's analysis of stereotypy. She uses as an index the number of repetitions of choice of stimulus cards after the shift from the no-solution problem to the position problem. By this measure the slow learners were much more stereotyped than the normal learners.

All in all, Marquart's experiment is very instructive. Although the frustrating effects of the punishment in the no-solution situation were distinctively evident in only 11 subjects, these effects were quite definite and can hardly be attributed to anything other than frustration or something similar to frustration. The contribution of stereotypy or lack of flexibility to the inefficient problem solving of the susceptible subjects, noted also by Guetzkow and others, can now be considered well established.

Similar to Marquart's study in principle but altogether different in design is one by Gordon & Durea (8) on the effect of discouragement. Instead of inventing new problems for their subjects, they used the Stanford-Binet Intelligence Test, which consists largely of minor problems to be solved. High school students, 20 in a control group and 20 in an experimental group, took Form L and later, Form M. The experimental group did little word problems just before taking the second form and were told by the experimenter that their performance was poor. While taking the test itself, they were told that they were "doing terribly." Mean score of the control group increased from one form to the other, but for the experimental group the mean decreased. The difference was significant. So, once again, we see how emotional factors affect intellectual performance, and we have empirical justification for the test manuals' instructions to encourage the subjects being tested.

Bouthilet (9) has made a clever experimental attack on a problem that experimental psychology ordinarily evades, the problem of intuition. Ostensibly, her subjects had to learn word pairs, but the number of word pairs was so large and the rate of presentation so rapid that little improvement was possible, unless they caught on to a hidden relationship between the words. Seven of the 20 subjects showed no improvement, while the curves for the other 13 climbed sharply. The difference between the two types of curve is undeniable.

Bouthilet's stated purpose was the "measurement of intuitive thinking," which she interprets as making correct guesses without knowing why. Indicating intuitive thinking in this sense, she says that six of the 13 subjects who improved started to make correct responses before their general behavior showed that they were aware of the hidden relationship. The evidence for this important statement is thin—mere observation of rate of responding, gestures, and spontaneous comments. The history of psychology has recorded the difficulty of establishing subjective phenomena such as awareness of something; establishing its absence, therefore, especially at a critical moment of time, will be equally difficult.

Youtz (10) reported briefly at the Boston meeting of the American Psychological Association an experiment on the effect of previous experience on problem solving. His subjects were 80 college girls, four groups of 20 each. They began by working short problems of the arithmetic reasoning type, all of which could only be solved by one method, which Youtz called hypothesis A. The different groups worked 5, 10, 20, and 40 of these A problems. After this practice all were given 10 problems that could only be solved by another method, called hypothesis B.

The practice had the expected result on the A problems in that time of solution decreased smoothly and, at the end, those with the most practice worked the problems in the shortest time. But when they shifted to the B problems the effect was reversed; the groups with the most practice on the A problems were slowest on the B problems. The experiment is clear-cut, and the differences are significant. "One determiner of resistance to a new hypothesis is number of previous successes on an incompatible hypothesis." Or, to put it in another frame of reference, problem solving methods may transfer to new problems, and the transfer effect may be negative.

An experiment reported by Grant & Berg (11) yielded results superficially contradictory to those of Youtz. They worked with a card sorting task that was a modification of the Weigl-Goldstein-Vigotsky concept formation test. Sorting on the basis of color was called "right" at first; then, without explanation, number classifications were called "right" and the others, "wrong." In a similar way, the basis of correct classification was shifted to form, back to number, then, to color, and finally, to form again. Seven groups of college students were used as subjects, one of which was allowed three correct trials with each classification before the shift to the next; another was allowed four; another, five, and so on to

ten confirming or reinforcing trials. It turned out that the groups with larger numbers of preshift reinforcing trials made fewer errors, both perseverative and nonperseverative errors, after the shifts. In this experiment the effect of increased reinforcement of the old solutions was to facilitate new solutions.

Since these two experiments are so similar in principle, it must be the differences in the kind of problem and the procedure that resolve the paradox. A solution to one of the Grant & Berg problems was a simple response, placing a card on one of four piles, whereas a solution to one of the Youtz problems was a relatively complicated arithmetical operation. Furthermore, Grant & Berg required their subjects to shift five times, and their analysis shows that the effect of increased reinforcement was statistically significant only after the fourth shift. Evidently, their subjects learned to respond to sequences, to be sensitive to shifts in the pattern of reinforcement. In other words, they learned a higher order pattern of response, and there was no interference with this higher order habit. It is possible that if Youtz (10), after making his subjects shift from hypothesis A to hypothesis B, had made them shift again to hypothesis C, then, to hypothesis D, and so on, they also would have developed a capacity for shifts of an even higher order; hence, the interference effect would have disappeared. Evidence for higher order habits in apes, children, and adults that transfer to new problems in spite of possibilities for interference is presented in Harlow's paper (reviewed in this volume by Melton, see p. 14).

## Individual Differences in Problem Solving and Judgment

Several of the experiments described above have paid some attention to individual differences in ability to solve problems. Terms like stereotypy, rigidity, perseveration, inflexibility, and inability to shift have been in use for some time. Since most of the tests in this area are scored only qualitatively, Berg (12) recommends the card sorting test described above—now called the Wisconsin Card Sorting Test—as a source of objective scores for statistical analysis. A wide range of scores is obtained from this test, and older people are said to be much less flexible than college students.

The topic of flexibility was approached in a very different manner by Kleemeier & Dudek (13). They raised the question whether factor analysis of a variety of tests would disclose a flexibility factor. Their tasks were not of the sort discussed in this review but could be called alternation tasks. Their subjects had, for example, to add, to subtract, then to do both alternately. The test battery was built around the numerical, verbal and perceptual speed factors, and, after rotation, these well-known factors appeared, but no flexibility factor was necessary. These results are an addition to our knowledge of alternation tasks but should not discourage anyone who seeks a flexibility factor in more complex tasks of the problem solving type.

Factor analysis seems to have the lead today—as systematic experimental introspection had in an earlier day—in the search for differentiable thought processes. The most impressive analyses of this kind were accomplished by the Aviation Psychology Program of the Army Air Forces and were published recently in Report No. 5, *Printed Classification Tests*, edited by Guilford & Lacey (14). Several of the investigations reported therein are pertinent to our topic. The centroid method of analysis was followed, and the rotations were orthogonal.

One study concentrated on tests of change of set. In all eight of these tests, the first few problems establish a set which becomes a handicap on subsequent problems. When the battery was given to 333 aviation students, the intercorrelations were all under 0.20. Either the tests were not reliable or, more likely, ability to change set, when measured in this way, is not a general ability. Guetzkow's results (6) with two tests of ability to surmount set are in line with this conclusion.

This report also describes a factor analysis of reasoning tests based on the scores of 22 tests given to 202 subjects. At least one general reasoning factor was identified; in it are involved such diverse tests as mathematical reasoning, spatial reasoning, competitive planning, and reading comprehension. All reasoning tests are complex factorially, with considerable undefined nonerror variance. The general reasoning factor and the familiar verbal factor are both quite general, appearing in many tests, but the two are independent.

Another factor analysis by Air Force psychologists included tests of judgment as well as reasoning because "good judgment" had been emphasized by flight instructors. Most of the judgment tests require planning; a tight situation is described and the subject has to select the best of several alternative ways of getting out of the predicament. The tests were given to 689 high school students and 1024 aviation students. The general reasoning factor appeared again, and perhaps another reasoning factor, but a weak factor called practical judgment was also identified.

Another volume about the Aviation Psychology Program, Report No. 6, *The AAF Qualifying Examination*, edited by Davis (15) describes a factor analysis of the scores from the 689 high school students carried out by the principal-axes technique. Like the centroid method this technique emphasized a general reasoning ability which is said to be "probably closely akin to the principal mental function measured by ordinary intelligence tests," and a common sense or judgment factor. In addition, some other factors called deductive reasoning, syllogistic reasoning, and reasoning in reading were identified.

These factorial studies raise as many questions as they answer. It is no surprise that reasoning, judgment, and planning are factorially complex, and that clear definitions of these functions do not emerge from factor analysis. Unless reasoning, for example, is given a highly restricted meaning, it is necessarily complex and not just another independent ability to be added to the list; but one may hope that factorial techniques

will be able to handle such complexity. Apparently speed is important on all these tests; the time devoted to any one problem must be short. But it is a safe guess that the general reasoning factor, including practical judgment, would help a laboratory subject, to an unknown extent, in solving the conventional, unspeeded, laboratory "thought problems." The test items are clearly illustrated in these reports, the statistics are given in detail, and the samples are large. In spite of the inconclusiveness of factor analysis, careful study of these two volumes will contribute a good deal to anyone's understanding, both theoretical and practical, of the higher intellectual abilities.

Thoughtful teachers have always attempted to encourage serious thinking in the classroom and, in recent years, to test for thinking or reasoning as opposed to memorization. A carefully executed investigation by Dunning (16) will illustrate this approach. He devised reliable tests of "scientific thinking" in college physics, one part called "interpretation of data," the other, "application of principles." When given to 109 freshmen, the correlation between the two parts was 0.48. The test as a whole showed low correlations with a reading test, with the various parts of the Primary Mental Abilities battery, and with scientific interest as measured by the Strong Vocational Interest Blank, but a correlation of 0.62 with a test of factual information in physics. The author takes this correlation as justification for testing "scientific thinking" independently of factual information; it could also be taken as support for the common textbook statement that the first requirement of good thinking is some remembered facts.

Recent interest in general principles of judgment provoked Frawley's factor analysis (17) of anchoring effects. He had 115 seminarians rate on a seven point scale 10 items from each of 15 Thurstone Attitude Scales. The procedure was repeated a week later with one end of the scale anchored. That is, category 7 was defined by an extreme attitude statement for each scale. Anchoring the scales in this way shifted the ratings, as would be predicted from general principles of anchoring, some judges of course shifting more than others. When average shift scores or anchoring effects on the 15 scales were intercorrelated in this sample of 115 subjects, the correlations were practically all positive. Factor analysis by Moore's modification of the tetrad technique disclosed general factors and a superfactor, indicating "a common matrix of causality." The interpretation of this general factor is less definite. Frawley suggests that his subjects might be differentially but consistently sensitive to the shock of the extreme anchoring statement. This factor might also be interpreted as a general factor of perseveration or resistance to change, or in terms of differences in retention over a period of a week. Nothing that could be called a measure of good judgment was discovered in the data; correlations of anchoring effect and intelligence were mostly negative, but not far from zero.

### The Development of Concepts

Concepts and the conceptualization process have a double significance for the psychology of problem solving. Certainly, the solution of many of life's problems depends upon the previous mastery of verbal and numerical concepts, such as words, mathematical symbols, and scientific principles. And, in reverse, the mastery of a difficult concept may be a problem in itself, the solving of which is well worth psychological investigation. The conceptualization process may be brought into the laboratory by presenting the subject with a variety of objects or designs on cards and asking him either to learn the symbols by which the experimenter has arbitrarily designated the various classes of objects (the memory method) or to sort the cards into piles representing classes (the sorting method).

Heidbreder, over a period of years, has reported a series of experiments on concept formation, beginning with the memory method and later switching to the sorting method. Her general question is: "Why are some concepts easier to master than others? In previous experiments of this series, Heidbreder and her students exposed drawings in a memory apparatus, following Hull's procedure, and asked college students to learn nonsense syllables for the various classes of drawings. The principal result, up to the present, is that under certain conditions concepts of concrete objects are attained most readily with concepts of spatial forms coming next, and numerical concepts last. The differences are significant, and the obvious secondary cues have been ruled out. The same order of attainment has since been reported by Wenzel & Flurry (18). Concepts of color are not definitely ranked by this procedure; they usually, but not always, are between form and number in order of attainment.

To account for these results, Heidbreder has put forth the hypothesis that the perception of concrete objects is the dominant mode of cognitive reaction, related to the phylogenetic priority of locomotive and manipulative capacities of the organism, and that conceptualization is, therefore, easier the more it resembles the perception of concrete objects. The history of thought yields examples which fit in neatly with Heidbreder's thesis. The germ theory of disease caught on quickly, but a concept like homeostasis diffuses slowly. Atomic energy is conceived by most people, not as a system of energy relationships, but as a concrete object, a gadget.

However, when Heidbreder and her collaborators (19 to 23) extended the experiments, including instances in which mutual interference was possible, making use of the sorting method, and presenting verbel phrases instead of drawings, the previously regular order of attainment was no longer so apparent. No doubt human conceptualization is very complex and under different conditions may proceed by different routes. Any one tendency, unless exceedingly dominant, will often be obscured by other tendencies. Some of these other tendencies which operate to facilitate and to impede concept formation are brought out in the later Heidbreder studies.

The Wisconsin Card Sorting Test uses 64 cards that can be sorted according to color, form, or number. Its use by Grant & Berg (see p. 301) has already been mentioned. It lends itself, also, to a partial check on the Heidbreder hypothesis. No concrete objects are included, but color, form, and number can be compared in respect of difficulty. Jones & Grant (24) made such a comparison and found, contrary to Heidbreder, that number concepts were apparently the easiest. A repetition of this experiment by Grant (25) with better control of form and number showed that the number concepts, which appeared to be easy, were being treated by the subjects as form concepts. Arranging the same numbers of designs in different configurations demonstrated that number concepts were actually utilized in sorting less readily than color or form concepts. In these respects Heidbreder's hypothesis is upheld. Grant agrees that mastery of number concepts proceeds more slowly than mastery of color and form concepts because the latter are more directly perceptual, while the former requires more analysis.

Dixon (26) raised the question whether children might use concepts like "big" and "little" without knowing the relation between them. He had 143 children of age two to six identify big and little cards of various shapes as a test of their knowledge of these concepts. The two cards of the last pair were identical except in color but, because of the well-known ring-segment illusion, the lower one looked larger. The experimenter asked each child to give him the big one, then, reversed their positions and repeated the instructions. Expressions of surprise or confusion, spontaneous verbalization, and the like, as rated by two observers, were taken as evidence of recognition of the contradictory relationship. Analysis of the results showed that over 90 per cent of the children who demonstrated functional mastery of the concepts reacted positively to the illusion. There was no evidence that the relation between the concepts was acquired later than the concepts by themselves. This finding is said to support Spearman's statement that relations are essential aspects of concepts.

A critic might question the use of a two category procedure in this experiment. Only two names are used, "big" and "little," and only two responses are permitted. If the child does not select one card he must select the other. The relationship of opposition is thereby emphasized immediately before the critical test. The use of the illusion to test knowledge of the relationship would be more convincing if divorced somehow from the previous practice. Perhaps the chief contribution here is a new technique for getting into communication with young children's conceptual systems, similar to the well known absurdities test.

Under the general heading of concept formation Brotherton, Read & Pratt (27) have reported an interesting study of school children's use of indeterminate number concepts. By this term they mean such semi-quantitative or indefinite concepts as "hardly," "several," and "a lot." Their general purpose was to find out how well children of different ages will use these concepts in referring to various numbers of objects. In-

stead of three-dimensional objects, they used printed symbols: Ó, A, $, h, *, (, Y. Various numbers, 3, 8, 13 to 33, of these symbols were printed in rows, and each group of symbols was to be checked by the students in one of these six indefinite number categories: hardly or scarcely any, few, several, some, lot, many. Completed papers were secured from 129 children in Grades 2 and 3, 89 in Grades 6 and 7, and 118 in Grades 10 and 11, but because of omissions, especially in the lower grades, and other selective factors, the authors do not claim that their samples are representative.

The results are presented in tables of percentages indicating frequency of use of each concept for each number, e.g., the frequency of use of "some" for 23 A's, of "many" for 18 *'s, etc. Here is a large mass of data of potential, practical and theoretical significance, but, unfortunately, Pratt and his students believe it "impossible to define the indeterminate numbers in terms of central tendency nor in those of the conventional expressions of dispersion." Hence, they are forced to summarize their data in several pages of very specific comparisons. "For groups of eight symbols Grades 6 and 7 and Grades 10 and 11 favor *few* and *several*, whereas Grades 2 and 3 prefer *some*." Such isolated comparisons are not enlightening and may, in fact, be misleading, because the use of any single concept is not independent of the context established by the other five of the set.

It is possible to summarize the results and reach some general conclusions by means of chi-square. The frequencies for the six indeterminate concepts applied to the seven numbers can be put into a six-by-seven table, suitable for the calculation of a measure of association. When this was done by the reviewer, the outcome was a contingency coefficient of 0.53 for Grades 2 and 3, 0.74 for Grades 6 and 7, and 0.73 for Grades 10 and 11. For these subjects, then, an increase in precision of use of these number concepts is evident from Grades 2 and 3 to Grades 6 and 7 but not beyond that point.

The authors' method of analysis leads them to stress their subjects' lack of consistency in the use of these terms.

> This investigation has demonstrated the variability in meaning of indeterminate number names. . . . In the light of these findings it may be concluded that personality inventories, study habits questionnaires, and other devices, which evaluate the characteristics of persons in terms of responses to items embodying indeterminate number names as crucial components, are invalid and unreliable.

The data do not justify such a strong conclusion. Contingency coefficients of 0.73 and 0.74 indicate at least a moderate relationship between the indeterminate number names and the absolute values of the numbers. The relationship is unnecessarily reduced by the inclusion of similar terms, like "several" and "some," and "lot" and "many," in the same set of response categories. The data really show that in the upper grades these concepts have reasonably good communicative utility; inventories cannot be fairly criticized by this method.

## Words

Words are symbols of particular significance for human relations as well as for an understanding of symbolic processes. Experiments on word association have an important place in the history of our topic, and there is a hint of a revival of interest but with improved methodology and closer relations with other areas of psychology (28 to 31). Razran (32), in fact, has employed practice in controlled association as a technique for establishing a desired set. Individual differences in the use of words in communication has come in for some study (33, 34), and verbal fluency continues to interest several investigators (35, 36, 37).

Dougan, Schiff & Welch (38) have reported a contingency coefficient of 0.60 between various fluency scores and supervisors' ratings of the originality of 33 employees of the display department store, and Thorndike (39), on the basis of college graduates' definitions of artificial words, has made some interesting observations on the psychology of invention.

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# AUTHOR INDEX


## A

Abrahamsen, D., 192
Abt, L. E., 155
Ackerman, N. W., 227
Adams, M., 258
Adams, S., 155
Ades, H. W., 42, 177
Adkins, D. C., 291
Adrian, E. D., 81
Agard, F. B., 247
Aita, J. A., 210
Albee, G. W., 224
Aldrich, C. N., 263
Alexander, F., 189, 228
Allen, E. B., 189
Allen, L., 3, 98
Allen, R. M., 216
Alliez, J., 202
Allport, G. W., 14, 15, 263
Allport, N. E., 137
Alper, T. G., 27, 112, 114, 194
Alster, B., 240
Altus, W. D., 88, 211, 215, 216, 261, 262
Ames, L. B., 1, 7
Ammons, R. B., 89
Anastasi, A., 87, 92
Anderson, G. L., 241, 242, 245
Anderson, R. G., 259
Andrews, T. G., 36, 80
Angoff, W. H., 162
Anscombe, F. J., 284, 293
Arcieri, L., 202
Arieff, A. J., 76, 77, 192, 196
Armington, J. C., 185
Asch, M. J., 228
Asch, S. E., 36, 121
Ash, P., 159, 218
Ashby, M. C., 192
Asher, E. J., 154
Ashford, T. A., 258
Aspin, A. A., 281
Atwell, W. O., 2
Atkinson, J. W., 110, 122
Austin, L. N., 267
Ausubel, D. P., 191
Axelrod, D. W., 275
Axelrod, J., 241

## B

Bachem, A., 58
Bakan, M. B., 27
Baker, H. De H., 40
Baker, K. E., 33
Baker, L. M., 308
Bakke, E. W., 153
Bakwin, H., 100
Baldwin, A. L., 109, 133, 240
Bales, R. F., 141
Ball, S., 42
Banerjee, K. S., 280
Banks, C., 95
Baratta P., 38
Barber, K., 144
Bard, P., 176
Bare, J. K., 84
Barker, M. E., 250
Barr, A. S., 250
Bartlett, F. C., 242
Bartlett, M. S., 281
Bartlett, N. R., 37, 41
Bartlett, S. C., 7
Baruch, D. W., 192
Bass, B. M., 160
Bateman, G., 284
Baumgardt, E., 43
Bavelas, A., 141
Bayles, S., 189
Bayley, N., 98
Beach, F. A., 174
Beamer, G. C., 158
Becker, F. C., *see* Corn-Becker, F.
Beebe-Center, J. G., 83
Békésy, G. v., 49, 50, 53, 56, 60, 64, 67
Bell, H. M., 257
Bell, J. E., 207
Bellak, L., 189, 191
Bellows, R. M., 151, 160
Bender, L., 189
Bender, M. B., 42, 77, 78, 176, 177, 217
Benedict, R., 109, 124
Benne, K. D., 141
Bennett, G. K., 87, 157, 257, 258
Bensley, M. L., 305
Benson, A. L., 257
Bentley, M., 290
Berdie, R. F., 268
Berelson, B., 130, 133, 136, 142
Berg, E., 301, 302
Bergman, P., 223
Berliner, A., 35
Berliner, S., 35
Berman, I. R., 4
Bernhardt, K. S., 99
Berry, R. N., 33, 42
Bettis, M. C., 229
Bienfang, R., 71
Billig, A. L., 107
Bills, R. E., 239
Bilodeau, E. A., 27
Bindra, D., 62, 63
Birren, J. E., 5
Bishop, G. H., 71
Bitterman, M. E., 37, 38
Bittner, R. H., 158
Bixler, R. H., 228, 272
Black, L. J., 65
Black, S. P. W., 79
Blackwell, H. R., 31, 32
Blair, G. M., 235
Blake, R. R., 216
Blankenship, A. B., 132
Bliss, C. I., 289
Blocksma, D. D., 227
Blodgett, H. C., 23
Bloom, B. S., 241
Blum, G. S., 105, 108, 200, 212
Blum, J. E., 21, 22
Blum, J. S., 178
Blum, M. L., 151, 168
Blum, R. A., 21, 22, 178, 179
Blumenfeld, W., 4
Boeder, P., 33
Bogoch, A., 79
Boice, M. L., 7
Bolanovich, D., 154
Bordin, E. S., 222, 264, 267, 268, 272
Borg, L. E., 131
Boshes, B., 179
Botterell, E. H., 183
Bouma, P. J., 39
Bouman, M. A., 43
Bouthilet, L., 301
Bowers, H., 250
Bowersbuch, M. K., 229
Bowker, A. H., 282
Bowles, J. W., Jr., 167, 207
Bowman, J. M., 189
Bowman, K. M., 216
Boyd, D. A., 79
Brady, E. H., 238, 241
Brady, M., 202
Braun, H. W., 180
Bray, C. W., 65, 151
Brazier, M. A. B., 157
Breese, F. H., 101
Brenman, M., 223, 230
Brennan, J. G., 40
Brennan, M. E., 228
Bridgman, C. S., 41
Brill, A. A., 189
Brodbeck, A. J., 197

Brodgen, W. J., 84
Bromiley, R. B., 179
Bronfenbrenner, U., 213
Brotherton, D. A., 7, 306
Brouwer, P. J., 248
Brown, C. R., 40
Brown, C. W., 151
Brown, D. D., *see* Denny-Brown, D.
Brown, D. E., 166
Brown, M. T., 274
Brown, R. R., 155
Brown, W. L., 23
Brownell, W. A., 14, 237, 241, 242, 243, 244
Brownlee, K. A., 280
Brozek, J., 37, 38
Bruch, H., 189
Bruner, J. S. 36, 121, 122
Brunswik, F. E., *see* Frenkel-Brunswik, E.
Bry, J., 228
Bucy, P. C., 177
Bugelski, B. R., 27, 278
Bühler, C., 208, 209
Burgemeister, B. B., 100
Burke, C. J., 11
Burnham, R. W., 39, 40
Buros, O. K., 292
Burrow, T., 190
Burt, C., 93, 97
Buxton, C. E., 27

## C

Calabresi, R. A., 212
Cameron, N., 190
Campbell, J. D., 227
Campbell, J. W., 164
Cantril, H., 120, 123, 127, 129, 146
Carnes, E. F., 224, 269
Carp, A., 216
Carpenter, A., 37, 38
Carr, A. C , 223, 272
Carter, J. W., Jr., 207
Carter, L. F., 122, 137
Cartwright, D. P., 114, 119
Cason, H., 192
Casperson, R. C., 35
Cass, W. A., Jr., 216
Cattell, A. K. S., 95
Cattell, R. B., 94, 95, 96, 141
Center, J. G. B., *see* Beebe-Center, J. G.
Centers, R., 127, 129, 164
Chadwick, L. E., 83
Chance, J., 216
Chapanis, A., 31, 41
Chapman, D. W., 114
Chapman, W. P., 197
Chapple, E. D., 157
Chein, I., 120, 125, 138
Chen, T. L., 6, 101
Chesler, D. J., 154, 159, 160, 257
Chesrow, E. J., 101
Chow, H. H., 6, 101
Chow, K. L., 178
Christensen, J. M., 167
Christiansen, C., 198
Churchman, C. W., 293
Chyatte, C., 261
Clark, B., 63
Clark, G., 183, 184, 185
Clark, J. H., 211
Clark, K. E., 166
Clark, R. A., 110, 122
Clark, R. E., 191
Cleveland, E. A., 156
Clothier, R. C., 151
Cobb, S., 157, 197
Coch, L., 140
Cochran, W. G., 289
Cofer, C. N., 88. 140, 216, 308
Coffin, M., 263
Coffin, T. E., 166
Cohen, J., 39, 40
Cohen, L., 159
Coleman, H. S., 37
Coleman, J. C., 107, 189
Collins, F. D., 42
Colm, H., 213
Coltharp, R. W., 228
Combs, A. W., 116, 222, 255
Combs, W. W., 222
Comrey, A. L., 155
Conn, J. H., 230
Connelly, G. M., 130
Conrad, H. S., 214
Cook, B. S., 27
Cook, E. B., 40, 41
Cook, S. W., 120
Coombs, C. H., 280, 286
Cooper, J. B., 39
Corn-Becker, F., 229
Corsini, R. J., 262
Cottle, W. C., 257
Cottrell, C. L., 37, 38
Cottrell, L. S., Jr., 127, 128, 132, 133, 139, 144, 152, 163
Cotzin, M., 228
Couey, F., 34
Courtis, S. A., 236
Covell, W. P., 65
Cowan, E. L., 223
Cox, K. J., 156
Craigie, H. B., 195, 196
Cramer, J., 77, 78
Crawford, B. H., 40
Crawford, M. P., 178
Critchley, M., 201
Cronbach, L. J., 218
Crowley, M. E., 25, 277
Crutchfield, R., 121, 126
Cunningham, L. M., 19
Curran, D., 195
Curry, E. T., 3

## D

Dallenbach, K. M., 81
Darley, J. G., 257, 267
David, F. N., 282
Davis, A., 128
Davis, F. B., 243
Davis, H., 04, 65, 66, 67
Davis, H. V., 107
Davis, L., 179
Davis, W. A., 3, 101, 236
Dawson, J. G., Jr., 211
Delgado, J. M. R., 183
Dennis, W., 179
Denny, M. R., 18
Denny-Brown, D., 183
Deri, S., 139, 207, 212
Despert, J. L., 201
Dethier, V. G., 83
Deutsch, A. L., 229
De Vinney, L. C., 127, 128, 132, 133, 139, 144
DeVore, J. R., 32
DeVries, H., 51, 53
Dewees, S., 198
Diamond, S., 259
Diggory, J. C., 115
DiMattia, A. L., 50
Dimmick, F. L., 41
Dinerman, H., 136
Dinnerstein, D., 139
Dix, M. R., 67
Dixon, J. C., 34, 306
Dixon, W. J., 285
Dodd, S. C., 286
Dollard, J., 125, 224
Domarus, E., 222
Donahue, W. T., 259
Donner, K. O., 42
Doob, L. W., 125
Doppelt, J. E., 101
Dorcus, R. M., 78
Dougan, C. P., 157, 308
Doughty, J. M., 51, 54
Drake, L. E., 216, 261
Dudek, E. E., 160
Dudek, F. J., 90, 245, 302
Duffy, E., 116
Duncan, C. P., 26, 180
Dunkel, H. B., 247, 248
Dunning, G. M., 304
Dunsdon, M. I., 102

Duntley, S. Q., 37
Durea, M. A., 300
Dvorak, B. J., 87
Dwyer, P. S., 289
Dymond, R. F., 225

E

Ebaugh, F. G., 189, 227
Eckerman, A. C., 164
Eckstrand, G., 166
Edinger, T., 174
Edmondson, L. D., 158
Edwards, A. L., 282, 285
Eells, K., 128, 263
Egan, J. P., 61, 62, 297
Ehrenfreund, D., 19, 21
Eisenberg, W. J., 155
Elbel, E. R., 2
Elkin, A., 27
Elliott, D. N., 308
Ellis, A., 213, 214, 229
Ellis, M. E., 291
Ellson, D. G., 167
Elmadjian, F., 185
Elsberg, C. A., 82
Embree, R. B., Jr., 98, 263
English, H. B., 229, 268
Ericksen, C. W., 26, 180
Ericksen, S. C., 9
Erner, F., 34
Eron, L. D., 200
Escalona, S. K., 114
Estep, M. F., 160
Estes, S. G., 222
Estes, W. K., 18
Evans, C. E., 166
Evans, M. C., 138
Evans, R. M., 31, 39
Eysenck, H. J., 93, 97

F

Fairchild, L., 189
Fantel, E., 227
Farber, I., 20
Farbro, P. C., 159
Farr, J. N., 165
Faw, V., 228, 239
Federer, W. T., 280
Fehrer, E., 308
Feifel, H., 5
Feindel, W. H., 75, 76
Feldman, F., 225
Feldman, M., 81
Felsinger, J. M., 11
Fendel, W. H., 182
Ferguson, G. A., 288
Ferguson, L. W., 161
Fernberger, S. W., 35
Ferraro, A., 198
Festinger, L., 144
Fidler, R. F., 196
Finesinger, J. E., 157, 197
Finney, D. J., 280, 282 285
Fisher, R. A., 289
Fisichelli, V., 229
Fiske, D. W., 90, 213, 217, 218
Fleischl, J., 144
Fleming, E. E., 212
Flesch, R., 144, 165
Fletcher, H., 55
Flowerman, S. H., 138
Flurry, C., 305
Fodor, N., 228
Foley, J. P., 87
Forer, B. R., 218
Forlano, G., 240
Foster, D., 81
Foster, H., 25, 26, 277
Foulds, G. A., 4, 5
Fountain, G., 77, 78
Fowler, E. P., Jr., 50
Fox, B. H., 34
Fox, C., 5, 101
Fox, W. H., 260
Franck, K., 260
Frank, L. G., *see* Gidro-Frank, L.
Frank, L. K., 208
Frank, R. L., 189
Frawley, P. J., 304
Fredericksen, N., 102
Freeman, E., 41
Freeman, H., 185
Freeman, H. A., 292
French, E. G., 88, 215, 257
French, J. R. P., 140
French, N. R., 62
Frenkel-Brunswik, E., 117, 120, 123, 136, 240, 246
Frey, von M. *see* von Frey, M.
Friedman, A. P., 189
Friedman, M., 292
Friend, J. G., 163, 263, 268
Frings, H., 83
Froehlich, C. P., 257
Fruchter, B., 94, 308
Fry, G. A., 40
Fulton, J. F., 178
Furst, W., 228

G

Gage, N. L., 258
Gagne, R. M., 25, 26, 277
Galambos, R., 65
Gamble, C. J., 190
Gardner, B. B., 155
Gardner, L. P., 101, 175
Garfield, S., 211, 216
Garner, W. R., 34, 51, 53, 54, 55, 57
Gasorek, K. A., 100
Gaudet, H., 130, 133, 136
Gebhard, J. W., 40
Gellhorn, E., 183
Gengerelli, J. A., 36, 288, 290
Gentry, G., 23
Gerbrands, H., 141
Gernandt, B., 42
Gerth, H. H., 143
Gewirtz, J. L., 308
Ghiselli, E. E., 151, 154
Gidro-Frank, L., 229
Giese, W. J., 157
Gilinsky, A. S., 114
Gill, M. M., 223
Gilliland, A. R., 3, 166
Ginsberg, R. S., 81
Ginsburg, B. E., 241
Gitelson, M., 198
Gladstone, A. I., 11
Gladstone, R., 250
Glaser, R., 245
Glazer, N., 134, 139
Glixman, A. F., 112, 194
Goetzl, F. R., 81
Goheen, H. W., 292
Gold, T., 51, 52, 53
Goldblatt, S., 78
Goldman, F., 108, 117
Goldschmidt, M., 81
Goldstein, K., 176
Goodenough, F. L., 87, 99
Goodman, C. C., 122
Gordon, D. A., 39, 40
Gordon, L. V., 300
Gordon, T., 158, 161, 227
Gory, A. E., 102
Gottlieb, J. S., 192
Gottschalk, V. H., 283
Gottsdanker, J., 144
Gottsdanker, R. M., 185
Gough, H. G., 192, 216, 261
Gould, L. N., 202
Grace, G. L., 240
Grace, W. J., 185
Graham, C. H., 32, 33
Graham, F. K., 217
Grambs, J. D., 239
Grand, H. G., 191
Grandine, L., 25
Granit, R., 42
Grant, D. A., 278, 279, 301, 306

Gray, F. E., 185
Gray, H., 101, 189
Graybiel, A., 63
Grayson, H. M., 209
Greco, M. C., 269
Green, E. L., 41
Gregg, A. E., 158
Gregg, J. R., 41
Gresham, M., 308
Grether, W. F., 37, 167
Grice, G. R., 17, 19, 21, 25
Grimm, C. H., 167
Gross, L., 228, 239
Grubbs, F. E., 287
Grummon, D. L., 227
Grünthal, E., 174
Guetzkow, H., 298, 303
Gunter, R. C., 32
Guilford, J. P., 87, 91, 155, 258, 290

## H

Haagen, C. H., 308
Haas, R. B., 269
Hadley, J. M., 156
Haefner, R., 257
Haggard, E. A., 163, 263
Haggard, E. P., 268
Haigh, G., 271, 272
Haire, M., 162
Haldane, J. B. S., 283, 284
Hallpike, C. S., 67
Halstead, W. C., 93, 217
Hamburger, F., 32
Hamilton, M., 292
Hamister, R. C., 216
Hamlin, R. M., 224
Hammer, E. R., 32
Hammond, K. R., 89, 134
Hand, T., 165
Hanes, R. M., 37, 40
Hann, E., 247
Hansen, M. H., 246
Hanson, H. B., 4
Harding, J., 120, 139
Hardy, LeG. H., 41
Hardy, V. T., 224, 273
Haring, D., 123
Harlow, H. F., 14, 22, 25, 176, 178, 179, 237
Harman, J. B., 74
Harms, I., 3, 249
Harper, R. S., 75, 83
Harrell, R. F., 99
Harrell, T. W., 151, 166
Harris, C. W., 90, 91, 247, 248, 291
Harris, D. B., 87
Harris, F. J., 164
Harris, J. D., 50, 59
Harris, M., 279
Harris, N., 130
Harris, R. E., 198, 216
Hart, C. W., 131
Hart, H., 146
Hartley, E. L., 123
Hartline, H. K., 40, 42
Hartridge, H., 34, 39
Hathaway, S. R., 216, 229, 261, 262, 271
Havighurst, R. J., 101, 236, 237, 240
Hawkins, J. E., 66
Hay, E. N., 160
Hayes, K. J., 26, 180
Hayworth, T., 165
Hebb, D. O., 174
Hecht, M., 33
Hecht, S., 43, 53
Heidbreder, E., 297, 305
Heidgerken, L. E., 242, 245
Heidler, J. B., 6
Heine, P. J., *see* Johns-Heine, P.
Helson, H., 38, 39, 59, 123, 129
Hendley, C. D., 43
Hendrick, I., 223
Henneman, E., 77
Henry, F. M., 54
Henry, J., 191
Henry, W. E., 144, 145, 155
Hertz, M. R., 210
Hess, E. H., 33
Heston, J. C., 260
Hickerson, G. X., 275
Hightower, N. C., 191
Hildreth, G., 2
Hilgard, E. R., 9, 10, 11, 14, 16, 19, 22, 27, 241, 297
Hill, A. S., 99, 250
Hinsie, L., 227
Hirsch, M. J., 32, 33, 41
Hirschberg, C., 189
Hirsh, I. J., 49, 60, 61
Hoagland, H., 185
Hobbs, N., 226, 228
Hoch, P., 190
Hochstim, J. R., 132, 284
Hoffman, A. E., 224, 270, 272
Hogrefe, R., 138
Holbourn, A. H., 39
Hollingshead, A. B., 128, 261, 263
Holt, R. R., 191
Holzinger, K. J., 90, 291, 292
Honzik, M. B., 3, 98
Hood, J. D., 67
Hoppock, R., 165
Horenstein, B. R., 27
Horowitz, M., 33
Horrocks, J. E., 257
Horst, P., 288
Horvitz, D. G., 279
Horwitz, W. A., 191
Hotelling, H., 289
Hovland, C. I., 145, 152, 165
Hsü, E. H., 6, 96, 97, 287
Hubbard, R., 42
Hughes, R. M., 210
Hull, C. L., 9, 11, 13, 18, 19, 21, 24, 26
Hunnicutt, C. W., 7
Hunt, H. F., 216, 262
Hunt, J. McV. 225, 274
Hunt, W. A., 88, 215, 229 257
Hunter, J., 185
Hunter, W. S., 7
Hunton, V. D., 80
Hurvich, L. M., 39
Husband, R. W., 156
Huston, P. E., 193
Huth, R. W., 89
Hyde, R. W., 203
Hyman, H., 132

## I

Imus, H. A., 41
Inhelder, B., 33
Irion, A. L., 27
Irwin, O. C., 1
Isaacs, S., 189
Israel, R. H., 203, 225
Ivy, M., 305

## J

Jacobsen, C. F., 178
Jacobson, A., 124, 198
Jameson, D., 39
Janis, I. L., 127, 128, 132, 133, 139, 144, 152, 163
Janowitz, M., 146
Jarrett, R. F., 154
Jasper, H., 185
Jastak, J., 216
Jaynes, J., 183
Jeffress, L. A., 61, 290
Jenkins, J. G., 263
Jenkins, J. J., 165
Jenkins, W. L., 79
Jenkins, W. O., 19
Jennings, H. H., 246
Jennings, L. S., 262
Jensen, G. V., *see* Vraa-Jensen, G.
Jenssen, W., 264

Jobe, F. W., 41
Joel, W., 212
Johansen, H., 67
Johns, E. H., 33
Johns-Heine, P., 143
Johnson, D. M., 59
Johnson, E. E., 155
Johnson, H. K., 189
Johnson, L. C., 216
Johnson, N. A., 203, 225
Johnson, N. L., 280
Johnson, P. O., 242, 292
Jones, C. E., 197
Jones, D. M., 245
Jones, F. N., 41, 183
Jones, H. E., 2
Jones, O. R., 306
Jones, R. F., 189
Judd, D. B., 39
Judson, A. J., 216
Jurgensen, C. E., 164
Juul, A., 66

## K

Kahn, D. F., 156
Kaldegg, A., 100
Kalish, D., 22, 23
Kallmann, F. J., 5, 190
Kaney, E. M., 228
Kanner, L., 189
Kant, O., 192
Kantor, R. E., 216
Kaplan, S. J., 23
Karpe, G., 42
Karpman, B., 189, 192, 202
Karwoski, T. F., 308
Katz, C. J., 191
Katzell, R. A., 163
Kauffman, P. E., 224
Kaufman, E. L., 38
Kavruck, S., 292
Kearl, B., 144
Keele, K. D., 78
Kelley, T. L., 292
Kelly, B. K., 280
Kelly, E. L., 213, 217, 218
Kempthorne, O., 280
Kendall, B. S., 217
Kendall, M. B., 292
Kendall, P. L., 143
Kendall, W. E., 259
Kendler, H. H., 13, 16, 17
Kennedy, J. L., 185
Kephart, N. C., 162
Kerr, W. A., 41, 158, 165, 261
Keys, J. W., 60
Keysen, A., 144
Kezur, E., 225
Kilby, R. W., 269
Kilpatrick, F. P., 285
Kimble, G. A., 27
King, E., 32, 37
King, P. H. M., 162
Kirchen, C. J., 283
Kirchheimer, B. A., 275
Kirk, S. A., 99, 250
Kirkner, F. J., 78
Kirkpatrick, M. E., 227
Klebanoff, S. G., 88, 215, 257
Kleemeier, R. M., 302
Klehr, H., 215
Klein, H. R., 191, 198
Klein, M., 189
Klineberg, O., 120, 145
Kloehn, N. W., 84
Kluckhohn, C., 123, 124
Klüver, H., 177
Knight, R. P., 230
Knighton, R., 185
Knoell, D. M., 90, 291
Knoll, H. A., 32
Knott, J. R., 192
Kobrak, H. G., 64, 67
Koch, N., 260
Kogan, K. L., 207, 216
Kogan, L. S., 279
Köhler, W., 177
Konopka, G., 228
Koomen, M., 32
Koskoff, Y. D., 179
Kossack, C. F., 291
Kracauer, S., 143, 146
Kramer, B. M., 137, 140
Krech, D., 121, 126
Krechevsky, I., 20, 21
Kriesberg, M., 130
Kris, E., 239
Krugman, M., 208, 227
Kryter, K. D., 166
Kubie, L. S., 223
Kunkle, E. C., 74
Kunst, M. S., 1, 107
Kuntz, J. E., 41
Kurtz, A. K., 156, 211
Kutash, S. B., 208
Kutner, B., 263

## L

Lamar, E. S., 43
Lane, B. M., 211
Lane, C. E., 58
LaSalle, H., 40
Laseau, L. N., 166
Lashley, K. S., 21, 22, 24, 177, 178
Lasswell, H. D., 142
Lauck, M. T., 249
Lauder, B. A., 82
Lavozik, D., 179
Law, S. G., 227
Lawrence, M., 64, 65, 67
Lawshe, C. H., Jr., 151, 155, 159, 160, 162
Lazarsfeld, P. F., 130, 132, 133, 136, 142, 143, 144
Lazarsfeld, S., 228
Lazarus, R. S., 211
Leaf, M., 227
Learned, J., 7
Leavitt, G., 144
Leavitt, H. C., 194
Ledermann, W., 291
Lee, H., 23
Leeper, R. W., 17
Lehman, H. C., 6
Leighton, D., 124
Leitch, M., 211
Le Messurier, D. H., 77
Lempert, J., 67
LeShan, L., 228
Leshin, N., 67
Lester, A. M., 291
Leukart, R. H., 158
Lev, J., 287
Levene, H., 283
Leverenz, H. W., 32
Levin, M., 202
Levine, L. S., 216
Lewey, F. H., 78
Lewin, H. S., 225
Lewin, K., 140, 141
Lewinski, R. J., 6
Lewis, D., 292
Lewis, D. R., 83
Lewis, N. D. C., 189
Lewis, T., 76
Lewy, A., 67
Leyzorek, M., 38
Liang, T., 34
Liber, B., 227
Lichtenstein, P. E., 84
Licklider, J. C. R., 49, 60, 61, 62, 63
Lindsley, D. B., 184
Lipkin, S., 224, 275
Lippert, S., 64
Lippitt, R., 16, 17, 140
Lipsett, L., 274
Lisansky, E. S., 202
Lit, A., 34
Livingston, R. B., 183
Lloyd, V. V., 33
Locke, B., 167
Loeb, M. B., 124, 198
Loeser, L. H., 228
Loevinger, J., 286, 290
Loftus, J. L., 240
Longstaff, H. P., 259
Loomis, C. P., 138, 263
Loraine, P. K., 280
Lord, M. P., 34

Lorge, I., 87
Louttit, C. M., 227
Lowenfeld, M., 213
Luchins, A. S., 227, 229
Luckiesh, M., 37, 38
Ludvigh, E., 34
Lumsdaine, A. A., 127, 128, 132, 133, 139, 144, 152, 163, 165
Lumsdaine, M. H., 127, 128, 132, 133, 139, 144, 152, 163
Lund, D. W., 74
Lüscher, E., 50, 58, 67

## M

MacAdam, D. L., 39
McBride, A. F., 174
McCalley, J. E., 107, 189
McClelland, D. C., 102, 110, 122
McConnell, T. R., 242, 245
McCormick, E. J., 162
McCormick, T. C., 286
McCorquodale, K., 10, 17
McCulloch, R., 196
McCulloch, W. S., 184
McCutchan, K., 23
McDonald, M., 237
McDowell, M., 230
Macfarlane, J. W., 3, 98
McGehee, W., 162
McGeoch, J. A., 24, 26
McGill, V. J., 193
McGinnies, E., 121
McGranahan, D. V., 142, 145
McGrath, F. M., 239
McGuire, C., 237
Machover, K., 207, 212
McIntosh, W. J., 250
MacKenzie, B. K., 138
Mackenzie, G. M., 243
McKinley, J. C., 216, 261
MacMillan, M. H., 154
McMurray, G., 79
McNeil, E. B., 229
McNemar, Q., 292
MacNichol, E. F., 42
Madden, E. H., 297
Madow, W. G., 281
Magaret, A., 216
Magoun, H. W., 183, 184
Maher, P. J., 74
Mahler, W. R., 161
Maier, N. R. F., 13, 194, 197
Malamud, R. T., 229
Malamud, D. I., 229
Malmo, R. B., 185, 197
Malone, T. P., 228
Mandell, M. M., 155
Manheimer, D., 132
Mann, J., 228
Marquart, D. I., 193, 299
Marston, W. M., 74
Mason, G. A., 112
Masserman, J. H., 189
Mauro, A., 79
May, M. A., 18, 20
May, P. R. A., 197
Mayman, M., 263
Mead, L. C., 167
Mead, S., 73, 83
Meehl, P. E., 10, 17, 216, 261
Meeker, M., 128, 263
Mencher, H. C., 17
Menninger, W. C., 189, 203, 227
Mensh, I. N., 88, 215, 257
Merrington, M., 284
Merton, R. K., 123, 138, 144, 145
Meurman, O. H., 82
Meyer, M. M., 209
Meyers, C. K., 50
Michael, R. E., 244
Michael, W. B., 90, 91, 290
Michaels, J. J., 192
Michels, W. C., 39, 59
Miller, G. A., 56, 57
Miller, H., 192
Miller, H. C., 107
Miller, H. E., 270
Miller, M., 33
Miller, N. E., 18, 110, 125, 193, 222
Milne, L., 40
Mintz, A., 168, 200
Misbach, L., 105, 192
Misiak, H., 40
Mold, H. P., 162
Molholm, H. B., 201
Moloney, J. C., 200
Moncrieff, R. W., 84
Monjé, M., 32
Mood, A. M., 279, 282, 285
Moon, P., 37, 38
Moore, T. V., 96
Moran, P. A. P., 287
Moreno, F. B., 228
Morgan, C. D., 111
Morgan, C. T., 185
Morton, R. A., 42
Mościcki, J. Z., *see* Zwislocki-Mościcki, J.
Moser, H. E., 237, 244
Mosteller, F., 292
Mote, F. A., 32
Mountcastle, V. B., 77, 176
Mowrer, O. H., 192, 221, 224, 239
Mueller, C. G., 32, 33, 284
Mueller, E. E., 228
Mueller, R. D., 32
Muench, G. A., 224, 275
Muhlhan, G. J., 26
Mullinger, M. A., 79
Munroe, R. L., 211
Munson, W. A., 55
Murray, E., 41, 228
Murray, H. A., 107, 111, 123
Musser, M. J., 78, 196

## N

Nagay, J. A., 161
Nahm, H., 248
Nair, K. R., 280, 281
Necheles, H., 81
Neilson, P., 97
Nevers, J. E., 2
Newcomb, T. M., 213
Newhall, S. M., 40
Newman, E. B., 60, 63
Neyman, J., 242
Nie, L. W., 79
Nielson, J. M., 198
Nissen, H. W., 175
Noether, G. E., 283
Nolan, J., 165
Noland, E. W., 153
Norman, J. P., 191
Northway, M. L., 99

## O

O'Brien, B., 31, 34
O'Brien, E. D., 31, 34
Oden, M. H., 102
Oehrwall, H., 84
Ogden, R. M., 10
Ogle, J. N., 33
Oldfield, R. C., 39
Olds, E. G., 287
Olson, W. C., 236
O'Meara, W., 241
Orlansky, H., 106, 124
Osgood, C. E., 25, 27
Ostrom, S. R., 259
Otero, J. M., 43
Otis, J. L., 154, 158, 257
Overholser, W., 227
Overstreet, P., 305

## P

Paillas, J., 202

Paster, S., 196
Pastore, N., 190
Paterson, D. G., 7, 37, 259
Patnaik, P. B., 284
Patterson, P. M., 82
Pattle, R. E., 72
Patton, R. A., 26, 180
Paulson, E., 280
Payne-Scott, R., 167
Pearson, E. S., 284
Pelner, L., 79
Penfield, W., 181
Pepinsky, H. B., 263, 264, 267
Pepitone, A. D., 139
Perlman, B., 228
Perlman, H. B., 67
Perloff, E., 165, 166
Peters, C. C., 280
Peterson, J., 240
Petrie, A., 202
Pfaffman, C., 32, 84
Pfeffer, A. Z., 202
Pfeiffer, J. B., 185, 199
Philip, B. R., 38
Phillips, E. L., 4, 225
Piaget, J., 33
Pickering, G. W., 76
Pickford, R. W., 39
Piéron, H., 34, 39
Pincus, G., 185
Pintner, R., 240
Piotrowski, Z. A., 210
Pirenne, M. H., 43
Plaza, L., 43
Politz, A., 284
Pollack, I., 50, 58, 60, 61, 62, 63
Popper, O., 66
Porter, E. H., 273
Porter, P. B., 180
Porter, P. P., 26
Porter, R. T., 192
Post, D., 35
Postman, L., 16, 19, 36, 59, 121, 297
Powdermaker, F., 272
Pratt, K. C., 7, 306
Prentice, W. C. H., 21
Preston, M. G., 38
Pritchard, B. S., 32
Pronko, N. H., 6, 167
Prout, C. T., 189
Pumphrey, R. J., 39, 51, 52
Putnam, T. J., 100

## Q

Quenouille, M. H., 281

## R

Raab, D. H., 42
Rabinovitch, R. D., 189
Raimy, V. C., 224, 270
Rand, G., 41
Rao, C. R., 289
Rashkis, H. A., 200
Raskin, N. J., 223, 271
Rasmussen, H., 64, 67
Rasmussen, T., 181
Raven, J. C., 4, 100
Rausch, O. P., 248
Razran, G., 25, 308
Read, J. M., 7, 306
Reader, N., 224
Reed, J. D., 42
Rees, J. R., 195
Reese, T. W., 38
Reid, D. D., 196
Reid, J. R., 190
Reinitz, A. H., 101
Reitan, R. M., 210
Remmers, H. H., 258, 261
Rexed, B., 77
Rhode, A. R., 263
Rhymer, R. M., 95
Richard, F., 39
Richardson, H. M., 155
Richardson, M. W., 155
Riesman, D., 134, 139
Riggs, L. A., 32, 33, 42
Rimoldi, H. J., 93
Riopelle, A. J., 291
Ripley, H. S., 185, 192, 199
Risser, J., 112
Ritchie, B. F., 22, 23
Rittler, M. C., 41
Robb, J. P., 181
Robinson, F. P., 224, 269
Robinson, I. P., 36
Robinson, J. T., 238
Roby, T. B., 110
Rock, M. L., 34
Rodgers, T. S., 192
Roe, A., 211
Rogers, C. R., 222, 223, 228, 229, 239, 240, 273
Rogers, S., 38
Rokeach, M., 117, 123, 136, 137
Romanoff, L. P., 185
Roscoe, S. N., 167, 168
Rose, A. M., 138
Rose, D. L., 73, 83
Rosenbaum, M., 123
Rosenblith, W. A., 49
Rosenthal, D., 140
Rosenzweig, L., 212
Rosenzweig, M., 63
Rosenzweig, S., 112, 194, 207, 208, 212, 216, 227
Rosner, B., 290
Ross, D. A., 50
Ross, H. V., 27
Ross, I. S., 228
Ross, S. J., 77, 78
Ross, W. D., 229
Rosvold, H. E., 180
Rothe, H. F., 154
Rothney, J. W. M., 246
Rothschild, T., 76
Rotman, D. B., 192, 196
Rotter, J. B., 208, 213
Roush, R. G., 32
Rubin, H., 216
Rudolf, G. deM., 198
Rudolph, L. M., 41
Ruesch, J., 124, 198, 222
Rulon, P. J., 168
Rundquist, E. A., 158
Russell, R. W., 26, 180
Ruth, J. M., 210
Ryan, T. A., 37, 38
Ryle, J. A., 198

## S

Salaverri, F., 43
Saltzman, I. J., 20, 24
Sander, G., 5
Sanford, N., 112
Sarason, S. B., 208, 211, 228
Sarhan, E. D., 100
Satter, G. A., 159
Saul, L. J., 189
Saunders, D. R., 290
Schachter, J., 308
Schaeffer, H., 34
Schafer, R., 207, 229
Schafer, S., 211
Schiele, B. C., 194
Schiff, E., 157, 308
Schiller, M. A., 76
Schiller, P. H., 175
Schilling, R. F., 78, 196
Schlismann, A., 308
Schlosberg, H., 35
Schlosser, J. R., 216
Schmidt, B. G., 99, 250
Schmidt, H. O., 216
Schmitt, O. H., 80
Schneidman, E. S., 212
Schneirla, T. C., 13, 174
Schofield, W., 224
Schonbar, R. A., 129
Schooler, E., 122
Schrader, W. B., 102
Schramm, W., 166
Schreiner, L. H., 184

Schuler, E. A., 138
Schulman, M. J., 101
Schuyler, G. L., 287
Schwartz, S., 123
Schwebel, M., 228
Scobee, R. G., 41
Scott, R. B., 33
Scott, R. P., *see* Payne-Scott, R.
Scott, W. D., 151
Seal, H. L., 282
Searle, L. V., 92
Sears, R. R., 106, 124
Seashore, C., 249
Seashore, H. G., 87, 157, 257, 258
Seashore, R. H., 264
Seeman, J., 224, 255, 269, 270, 272
Segal, J., 39, 50
Segall, G., 84
Seidenfeld, M. A., 229
Seitz, P. F. D., 201
Selover, R. B., 153
Semrad, E. V., 228
Sender, V. L., 34
Seton, P. H., 185
Settlage, P. H., 178, 179
Seward, J. P., 14, 15, 17
Shagass, C., 185, 197
Shakow, D., 193, 223, 226
Shapiro, S. L., 67
Shartle, C. L., 160
Shaw, F. J., 221
Shaw, W. A., 60
Sheats, P., 141
Sheerer, E. T., 270, 272
Sheffield, F. D., 152, 165
Sheldon, W. H., 191
Sherman, I. C., 76, 77
Sherman, M., 197
Sherriffs, A. C., 212
Shils, E., 125, 146
Shlaer, S., 43
Shneidman, E. S., 200, 227
Shoben, E. J., Jr., 193, 221
Shotwell, A. M., 250
Silverman, S. R., 66
Simmons, W., 284
Simon, A., 189, 216
Simonson, E., 37, 38
Simos, I., 274
Simpson, M. M., 216
Sims, V. M., 244, 261, 263
Sinaiko, H. W., 156
Sinclair, D. C., 75, 76, 77, 182
Sisson, E. D., 88, 161
Sivian, L. J., 53
Skeels, H. M., 3, 98, 249
Slater, P., 100, 289
Sleight, R. B., 41, 167, 168
Sloan, L. L., 39
Sloan, W., 211
Smith, A. J., 239
Smith, C. A. B., 283
Smith, D. M. K., 132, 284
Smith, G. H., 129
Smith, H. L., 191
Smith, I. M., 249
Smith, K. R., 36, 64, 67
Smith, L. F., 274
Smith, M., 154
Smith, M. B., 120, 126, 127 128, 132, 133, 139, 144, 152, 163
Smith, M. H., 19, 60
Smith, S., 32, 193
Snyder, W. U., 223, 224, 269
Snygg, D., 116, 222, 255
Solomon, R., 242
Solomon, R. L., 11, 27, 33, 277, 278
Soresi, A. L., 74
Soskin, W. F., 213, 217 218
Sparer, P., 229
Speier, H., 146
Spelt, D. K., 1
Spence, K. W., 10, 11, 16, 17, 19, 20, 21, 22
Spencer D., 37, 38
Spriegel, W. R., 151
Stacey, C. L., 242, 245
Stafford, J. W., 96
Stagner, R., 164
Stanley, W. C., 183
Star, S. A., 127, 128, 132, 133, 139, 144, 152, 163
Stein, M. I., 207, 208
Steinberg, J. C., 62
Steinitz, M., 228
Stekel, W., 190
Stendler, C. B., 237, 238
Stephan, F. F., 131
Stevens, S. S., 75, 83, 191
Stevens, W. L., 280, 285
Stewart, N., 101
Stock, D., 270, 272
Stogdill, R. M., 160
Stone, C. H., 249, 274
Stone, C. P., 26, 180
Stone, D., 185
Stone, F., 81
Stone, G. R., 10
Stouffer, S. A., 127, 128, 132, 133, 139, 144, 152, 163
Straker, D., 154
Straub, L. R., 185, 199
Strom, K., 224
Stromberg, E. L., 154
Strong, E. K., Jr., 257
Strother, G. B., 158
Stubbs, A. L., 42
Stuit, D. B., 258
Suchman, E. A., 127, 128, 132, 133, 139, 144
Sulzman, J. H., 41
Sumner, F. C., 33, 80, 115
Super, D. E., 151, 251, 255, 256, 259
Swanson, C. E., 144
Sweet, A. L., 37, 40
Sweetland, A., 195, 229
Swenson, E. J., 242, 245
Swineford, F., 282, 292
Szurek, S., 227
Symonds, P. M., 240
Snyder, W. U., 250
Synolds, D. L., 6

## T

Taba, H., 237, 240
Taft, R., 155
Tajen, C., 157
Talbot, S. A., 31
Tamalet, J., 202
Tansley, K., 42
Tarjan, G., 192
Tate, M. W., 94
Tatham, C. M., 99
Taylor, C. W., 94, 308
Taylor, E. K., 157
Taylor, J. C., 11
Taylor, W. G., 56, 57
Terman, L. M., 102
Teuber, H.-L., 42, 77, 78, 176, 177, 217
Thelen, H. A., 241
Therman, P., 77
Thetford, W. N., 224
Thiede, W. B., 261
Thomas, G. J., 58
Thomas, W. I., 121
Thompson, C. E., 163
Thompson, G. M., 211
Thompson, G. N., 198
Thomson, G., 291
Thomson, L. C., 34, 39
Thorndike, E. L., 9, 19, 21, 26, 243, 308
Thorndike, R. L., 4, 98, 101, 151
Thorne, F. C., 228, 229, 268, 275
Thurlow, W. R., 66
Thurstone, L. L., 87, 213, 290, 291
Thurstone, T. G., 87
Tieze, T., 192

Tiffin, J., 154, 168
Tigay, E. L., 76
Tinbergen, N., 175
Tinker, M. A., 37, 38
Tinker, M. L., 7
Tolman, E. C., 9, 12, 13, 14, 16, 18, 21, 22, 23, 26
Tomkins, S. S., 208
Toops, H. A., 291
Tousey, R., 32
Townsend, J. C., 26
Travers, R. M. W., 247, 273
Travis, R. C., 33
Tresselt, M. E., 59
Treuting, T. F., 185
Tucker, L. R., 291
Tucker, W. B., 191
Tudenham, R. D., 98
Tukey, J. W., 292
Turner, W. D., 89, 160
Tuthill, C. E., 308
Tyler, L. E., 87
Tyler, R. W., 236

## U

Uhrbrock, R. S., 153
Underwood, B. J., 13, 27, 297

## V

Valenstein, A. F., 228
Van Der Velden, H. A., 43
Van Lennep, D. J., 213
Van Vleet, P., 228
Varva, F., 229
Veniar, F. A., 35
Vernon, P. E., 101
Verplanck, W. S., 37, 43
Vogel, J., 153
Volkmann, J., 38, 75, 114
von Frey, M., 84
Votaw, D. F., 289
Vraa-Jensen, G., 66

## W

Waddell, D., 83
Wade, M., 24
Wagener, H. P., 201
Wagman, I. H., 40
Wagner, R., 153, 160
Wald, G., 42
Waldfogel, S., 112
Waldman, S., 79
Walker, E. L., 17, 33
Wallach, H., 63, 177
Wallen, R. W., 203, 211
Wallis, W. A., 292
Wallinga, J. V., 196
Walsh, J. E., 284
Walsh, T. E., 66
Walsh, J. F., 283
Walthall, W. J., 23
Walzl, E. M., 65
Ward, A. A., 176, 184
Ward, J. W., 184
Warner, W. L., 128, 144, 145, 263
Waters, R. H., 23
Wayne, I., 142, 145
Wayner, M., 42
Weaver H. E., 297, 298
Weaver, K. S., 40
Webb, W. B., 162
Webber, V. J., 7
Webster, H., 35
Weddell, G., 72, 75, 76, 182
Wedeking, L. P., 102
Wegel, R. L., 58
Weigle, F., 157
Weiss, E., 189
Weitz, J., 35, 230
Welch, B. L., 281
Welch, L., 157, 193, 229, 308
Welsh, G. S., 262
Wenger, M. A., 96
Wenzel, B. M., 80, 81, 305
Wepman, J. M., 217
Wesley, S. M., 209
Wesman, A. G., 87, 257, 258
Wever, E. G., 64, 65, 67
Weymouth, F. W., 32, 33, 41
Wheeler, E. T., 179
Wheeler, P., 81
Wheeler, W. M., 209
Wherry, R. J., 152
White, M. A., 199
White, S. D., 53
White, V., 248
Wickens, D. D., 25, 213
Wiener, D. H., 262
Wiener, D. N., 216, 225
Wiener, F. M., 50
Wiener, N., 185
Wilcoxon, H. C., 23
Wilks, S. S., 292
Williams, A. C., Jr., 167
Williams, J. C., 241
Williams, M., 88, 215, 257
Williams, R. M., Jr., 127, 128, 132, 133, 138, 139, 144, 152, 163, 246
Williams, S. B., 37, 40
Wilson, E. C., 144
Wilson, R. F., 160
Winder, C. L., 216
Wing, H., 7, 249
Winter, O. S., 185, 199
Wirth, L., 136, 137
Wishner, J., 211
Wispe, L. G., 96, 141
Withey, S., 27
Witkin, H. A., 36
Wittenborn, J. R., 208, 211
Wittman, P., 191
Wolberg, L. R., 195, 230
Wolf, S., 79, 185, 192, 199
Wolfe, J. B., 178
Wolff, H. G., 79, 185, 199
Wolff, W., 212
Wollach, L., 39
Woltman, A. G., 227
Wood, B., 257
Woolsey, C. N., 77
Worbois, G. M., 87
Wortis, S. B., 77, 78, 176, 177
Wosika, P. H., 101
Wren, H. A., 258
Wrenn, C. G., 264
Wright, D. G., 196
Wright, H. W., 267
Wright, W. D., 34, 39
Wulff, V. J., 42
Wyatt, F., 273
Wylie, R. C., 158

## Y

Yacorzynski, G. K., 179
Yamaguchi, H. G., 11
Yarrow, L. J., 111
Yates, F., 281
Yedinack, J. G., 7
Yoder, D., 151
York, R. H., 203
Young, P. T., 20
Youtz, R. P., 301, 302

## Z

Zaccaria, M. A., 41
Zaloski, F. C., 102
Zawadski, B., 137
Zegers, R. T., 33
Zilboorg, G., 228
Zimmerman, F. T., 100
Zimmerman, J., 229
Zimmerman, W. S., 87 258
Zipf, G. K., 199
Zlatchin, P., 165
Znaniecki, F., 121
Zwislocki, J., 50, 58, 67
Zwislocki-Mościcki, J., 65

# SUBJECT INDEX


## A

Ability, mental, testing of, 88–89
Ability, verbal, 89
Abnormal behavior, *see* Behavior disorders
Adjustment, work and family background, 163–64
Adolescence, mental test performance and, 101
Adrenal cortical hormones, schizophrenia and, 185–86
Advertising
  communications and, 165–67
  effectiveness of, 166–67
  *see also* Communications
Age
  color concepts and, 6–7
  developmental functions and, 6–7
  learning ability and, 7
  literary output and, 6
  mental organization and, 6
  musical ability and, 7
  number concepts and, 7
  psychological attributes of the aged, 101
  space concepts and, 7
  vocabulary quality and, 5
  vocabulary size and, 5
Alcoholism, 202
Altruism, measurement of, 89
Amphetamine, epilepsy and, 178
Animal behavior, 92
  genetic differentiation and, 92
  sex and, 174
  stimulation of, 181–83
  *see also* Animal psychology
Animal psychology, 173–86
  comparative method and, 173–76
    problems of, 174
  evolution and, 174–75
  instinctive behavior and, 173–74
  rate of learning and, 175
  structure and, 173
  *see also* Animal behavior
Anxiety
  behavior disorders and, 198–99
  blood pressure and, 199
  gastrointestinal illness and, 198–99
  physiological reactions to, 198
  psychosomatic factors in, 198
  psychosomatics, stress and schizophrenia, 185–86
  trend intensification and, 198
Aptitude tests, 87
  efficiency of, 258
  professional, 258–59
  special, 257–58
  *see also* Interests, tests of
Aspiration, level of
  instructions for judgment of, 115
Aspiration, level of (*cont.*)
  perception of ability and, 114
  personal experience on, 114–15
  sex differences on, 115
Attitude
  description of, 125–34
  educational level and, 128–29
  indicators of, 89–90
  latent meaning of, 134
  measurement of, 89, 125–34
  mental, testing of, 97
  of residents toward industrial nuisances, 166
  *see also* Attitude methodology; *and* Attitude theory
Attitude methodology, 130–34
  models, rationalization of, 133
  *see also* Opinion methodology
Attitude theory, 125–27
  Cantril's reformulation of, 126–27
  Doob's reformulation of, 125–26
  Harvard reformulation of, 126
  Krech and Crutchfield's reformulation of, 126
Attributes
  of children, 97
  measurement of, 87–90
  mental, of the aged, 101
  modifiability of, 97–100
  stability of, 97–100
Auditory flutter
  stages of, 57
  theory of, 57

## B

Behavior
  displacement and, 193
  electroconvulsive shock on, 180
  factor analysis of, 93
    cortical destruction on, 93
  maternal, electroconvulsive shock and, 180–81
  prefrontal association areas and, 179
  voting, 134–36
  *see also* Animal behavior; Behavior disorders; *and* Counseling methods, client behavior and
Behavior disorders, 189–203
  anxiety and, 198–99
  biosocial, 190
  brain damage and, 176–81
    agnosia and, 177–78
    comparisons of, 178–79
    emotion and, 176
    epilepsy and, 178
    perception and, 176–77
    variability of, 178
    vision and, 177
  enuresis and, 193

Behavior disorders (*cont.*)
etiology of, 190–92
experimental, 195
induced, 193–95
learning and, 192
obsessions and compulsions, 190
occupation on, 191–92
organization and, 192–93
pursuit learning and, 193
stress and, 195
sex drives on, 191
somatotypes of, 191
thinking and, 200
*see also* Behavior; *and* Deficiency, mental
Binocular vision, geometry of and depth perception, 32
Body, measurements of, 97
Brownian movement, hearing and, 53

## C

Centrifugal force, headache and, 74
Children
gifted, 102
hallucinations in, 201
Chi-square, 281–83
linear regression, test for, 281–83
smooth test, 282
two-by-two contingency problems, tables for, 282
Cinemanalysis, study of development by, 1–2
Class consciousness, origin of, 128
Clinical methods
projective techniques of, 207
psychodiagnostics and, 207–18
psychotherapy and, 221–30
Clinician, psychodiagnostics and, 217
Cognition
analysis of, 93, 101
perceptual factors in, 93–94
Communications
advertising and, 165–67
attention focus and, 142
content analysis of, 142
effectiveness of, 165
media of, 142–45
modern American trends in, 143
foreign characters and, 143
national trends and, 142–43
radio listening and, 143–44
radio serials and, 144–45
readership and, 165–66
text readability and, 144, 165
*see also* Literature
Concepts
chi square determination of, 307
concrete objects and, 305
difficulty of mastery of, 305
number, 306
Concepts (*cont.*)
number, indeterminate, 306–7
problem solving and, 305–7
size, 306
Contingency tests, *see* Chi-square
Continuity in learning, noncontinuity and, 20–22
Correlation, 287–91
multiple regression, 288
point-biserial, 287
reliability and validity, 287–88
rho, 287
zero order *r*, computation of, 291–92
*see also* Factor analysis; *and* Multivariate tests
Counseling methods
boundaries of, 267
case work and, 267, 273–74
client behavior and, 270
change in, 271
defensive, 271
hello-goodbye analysis of, 271, 275
maturity on, 271
diagnostic categories and, 264
diagnostics, 255–64
directive, 255
free situation, 255–56
Minnesota Multiphasic Personality Inventory and, 260–62
nondirective, 255
changes in, 269–70
on counselor, 270
limitations on, 268
problem solving and, 264
projective techniques in, 262–64
essay examination and, 263
social history and, 263
questionnaires, 274
sociometric techniques of, 263
test techniques, 256–57
therapy, 267–75
trends of, 256
variations in, 268–69
vocational diagnosis and, 256
*see also* Vocational guidance
Counseling therapy, 267–75
analysis of, 269–73
client attitude and, 272–73
client talk ratio and, 269
client test selection and, 272
continuity and, 274
counselor attitude and, 273
counselor personality and, 273
deterrents to, 269
effects of, 273–75
methods of measurement of, 274
grade achievement and, 275
initial interview and, 272–73
mental defectives and, 275
number of tests and, 272
personality and, 268

Counseling therapy (*cont.*)
rationalization and, 268
Rorschach test and, 275
theoretical contributions to, 268–69
Culture, personality and, 123–25

D

Deafness, 66–67
conductive and nerve, 66–67
fenestration operation and, 66
traumatic, 67
Decline
growth, development and, 1–7
of twins, mental and physical, 5–6
Deficiency, mental
in children, 249–50
*see also* Behavior disorders
Delinquents, intelligence of, 102
Delusions
in children, 201
fantasy, hallucination and, 200–2
Depth perception, visual functions and 32–33
Design of experiments, *see* Experimental design
Development
change of voice and, 2–3
fetal, conditioned response and, 1
growth and decline, 1–7
of language, articulatory handicaps and, 7
of motor functions, 1–3
of oral drives, 1
of personality, 105–9
of speech, 1
study of by cinemanalysis, 1–2
*see also* Age; *and* Growth
Dextrality, development of, 2
Diagnostics
counseling methods and, 255–64
therapy and, 264
value of, 255
Differential Aptitude Tests, analysis of, 257–58
Displacement, 110

E

Ears
interaction of, 59–62
masking and, 60
phase and, 60–61
mechanical properties of, 64–65
neurophysiology of, 65–66
toxicity of drugs on, 66
Education
character formation and, 238
disciplinary methods and, 239–40
general methods of, 245–46
guidance programs and, 248–49
individual programs of, 245
Education (*cont.*)
on IQ, 98–99
in nursing, 248
psychotherapy and, 238–39
reasoning and, 304
remedial, 250
spatial tests and, 249
specific methods of, 246–47
test methodology and, 243
training aids, wartime use of, 245–46
*see also* Educational psychology; *and* Learning
Educational psychology, 235–51
aptitude tests and, 248
comparative effectiveness of, 246
definition of, 235
foreign-language teaching and, 247
research methods in, 241–44
Engineering, human, 167–68
definition of, 167
dial legibility and, 167
visibility and, 168
Epilepsy, amphetamine and, 178
Experimental design, 277–80
experimental inference, theory of, 293
fractional replication and, 280
latin square in, 278–79
prime-power lattice designs, 280
sample size, determination of, 279
transfer of training, measurement of, 277–78

F

Factor analysis, 289–91
chance errors on, 290
communalities, algebraic solution for, 290
criticisms of, 290
differential calculus of matrices, application to, 290
new techniques of, 290–91
oblique factors, estimation of, 291
Fantasy
hallucination and delusion, 200–2
schizophrenia and, 200–1
Field-cognition, reinforcement versus, 15–20
Fixation, punishment on, 193–94
Fluency, 94
Forgetting, retention and, 26–27
Frustration, 197–98
neurosis and, 197
stereotypy and, 300
threshold of, 197

G

Glutamic acid, on intellectual development, 99–100
Group dynamics
education and, 241

Group dynamics (*cont.*)
individual perceptions and, 141
individual role behavior and, 141
productivity and, 140
study of, 140–42
syntality dimensions and, 141
Group processes
learning and, 240–41
psychotherapy and, 228–29
social psychology and, 119–47
sociometry and, 246
unit of analysis of, 120
*see also* Group dynamics
Growth
development and decline, 1–7
dextrality and, 2
IQ variation and, 3
reaction time and, 2
sex and, 2
*see also* Age; *and* Development
Growth, mental, 3–6
age trends and, 4–5, 6
environment and, 3
in infancy, 3
intelligence tests and, 4
poliomyelitis and, 4
socioeconomic differences in, 3–4
Guilford-Zimmerman Aptitude Survey, analysis of, 258

## H

Hallucinations
in children, 201
fantasy and delusion, 200–2
muscle potentials and, 202
olfactory, 202
schizophrenia and, 202
shipwreck and, 201
visual, ophthalmology and, 201
Hearing, 49–68
absolute threshold of, 49–51
variability of, 50–51
acoustical improvements and, 67
adaptation and masking, 58
atonal interval and, 50
auditory cortex responses and, 65–66
binaural interaction and, 59–62
chroma and, 59
cochlear response and, 65
diplacusis and, 61–62
energy levels and, 53
flutter in, 57
hallucinations of, deficient imagery and, 201–2
history of, 49
long time threshold of, 51
loss of, industry and, 168
masking and differential sensitivity of, 59
nerve potentials and, 65
neurophysiological aspects of, 65–66
Hearing (*cont.*)
new experimental techniques and, 67
perceptive loss of, 50–51
short tones
energy distribution and, 54–55
loudness of, 54
pitch and, 54
theory of, 55–56
thresholds, 51–52
sound localization and, 63–64
of speech, 62–63
theories of, 68
thresholds, energetics of, 52–53
tone duration and, 53–54, 55
*see also* Auditory flutter; Deafness; Ear; Noise; Sound; *and* Tones
Hunter-Sigler effect, span of vision and, 34–35
Hypnosis, hypnotherapy and, 229–30
neurosis and, 229
Hysteria, learning and, 193

## I

Ideology, opinion, values and, determinants of, 127–30
Imitation, suggestion and prestige, 121
Individuals
differences in, 87–102
race, nationality, age, sex and, 100–1
perceptions and group dynamics, 141
role behavior in groups, 141
traits of, 90
Industrial psychology, 151–68
accidents and, 168
classification tests, 153
co-ordination in, 152
criteria of performance, 161–62
employee selection tests and, 154
grievances and, 165
industrial nuisances and, 166
interviews and, 153
motivation and morale, 163–65
noise and, 168
personnel attitudes and, 164
Rorschach test and, 155
test procedures and, 153–58
training and, 162–63
*see also* Job analysis; *and* Personnel
Intelligence
adolescence and, 101
age curve and, 100–1
changes in, education on, 98–99
of delinquents, 102
development of
glutamic acid on, 99–100
thiamine on, 99–100
lobotomy on, 179–80
occupation and, 101
old age on, 4–5
quotients, 98
scales, abbreviated, 257

Intelligence (*cont.*)
testing of, 88–89
in children, 97–98
speed of performance, 94
*see also* IQ
Interests
stability of, 260
tests of
expanding use of, 259–60
fakability of, 259
masculinity-femininity scales and, 260
occupational characteristics and, 259
scholastic success and, 259–60
vocational, job choice and, 259
IQ
of children, 98
of college students, 98, 263–64
environment on, 98
of foster children, 98

## J

Job analysis
critical requirements and, 249
evaluation and, 158–61
Job evaluation
individual methods for, 159–60
scales for, 160
National Electrical Manufacturers Association System for, 160
National Metal Trades Association method for, 159
Occupational Characteristics Check List for, 160
paired comparisons method of, 159
specification scoring key of, 159
Judgment
adaptation level and, 123
anchoring effect on, 304
individual differences in, 302–4
perception and, studies on, 121–23

## L

Language, thinking and, 199–200
Latin square, analysis of, 278
Learning, 9–27, 192–93
behavior disorders and, 192
continuity in
controversy over, 21
cue reversal and, 21–22
discrimination learning and, 22
experiments on, 21
versus noncontinuity, 20–22
criteria of, 243
discrimination, 14–15
electroconvulsive shock on, 26, 180
emotional factors in, 238–40
evaluation of, 247–48
experimental approach to, 15
field theory and, 14
group processes and, 240–41
Learning (*cont.*)
growth and, 236
latent
experiments on, 16–17
reinforcement and, 16, 20
satiated drives and, 17–18
literary comprehension and, 247
meaningful, 244–45
measurement of, 242–43
place versus response, 22–24
play therapy and, 239
praise and criticism on, 240
reinforcement and, necessity of, 16
relationship to authority and, 240
retroactive inhibition and, 27
sets, character of, 15
social status on, 237
stimulus-response theory and, 13–14
test variation and, 248
transfer of, 24–26
approaches to, 24
measures of, 277
primary stimulus generalization and, 24–25
secondary stimulus generalization and, 25
study of by transfer effects, 25–26
types of, 13–15
*see also* Education; *and* Learning theories
Learning theories
animal experimentation and, 14
basic differences of, 10
conceptual structures of, 10–11
experience variable and, 15
field, 10
environmental stimuli and, 12
secondary reinforcement and, 19
Tolman and, 11
transfer of learning and, 12
multiplicity of, 9
new variables and, 13
problems of, 12–13
status of, current, 9–13
stimulus-response, 10
habit and, 11
Hull and, 11
inhibitory influences and, 11
secondary reinforcement and, 19
stimulus generalization and, 11–12
systematic experimentation and, 14
Literature
American trends in, 143
*see also* Communications
Loudness
masking and, 61
pitch and volume, 58–59

## M

Maturity, strength and, 2
Memory, *see* Retention

Mental abnormalities, *see* Behavior disorders
Minnesota Multiphasic Personality Inventory
  achievement scale of, 261
  attitude and, 262
  coding system for, 262
  counseling uses of, 260–62
  K-scale of, 216
  obvious and subtle scores of 261–62
  occupations and, 261
  scoring methods for, 262
  Social Introversion-Extroversion scale of, 261
  socioeconomic status and, 261
  validity of, 262
Morale
  in the army, 163
  factors in, 164
  family background and, 163–64
  job satisfaction and, 165
  motivation and, 163–65
  supervision and, 163
Motivation, morale and, 163–65
Multivariate tests, 288–89
  analysis of dispersion technique, 289
  compound symmetry, 289
  suppressor variables, 289
  *see also* Correlation

## N

Nerves
  electrical stimulation of, 72
  ischemia and, 76–77
  pain and, 182–83
  paralysis agitans and, 184
  stimulation of
    frequency and intensity of, 183
    motor responses to, 183
  thalamic projection of, 77
  types of, 182
  *see also* Suppressor systems
Neuropsychiatry, stress and, 195
Neurosis
  battle, 195–96
    casualty rate and, 196
    in fliers, 196
  pain, thresholds of, 196–97
  self-inflicted injury and, 196
Noise
  bursts of
    intensity and duration of, 56–57
    masking of, 57
    short tones and, 53–58
  deleterious effects of, 64, 168

## O

Opinion determinants
  information and enlightenment as, 129–30
Opinion determinants (*cont.*)
  interaction of, 130
  of values and ideology, 127–30
Opinion methodology, 130–34
  area versus quota sampling, 284
  eliminating callbacks, 284
  error-choice technique, 134
  interviewing bias and, 131
  interviewing problems and, 130–31
  panel techniques and, 132–33
  question wording and scale construction, 133–34
  radio and, 144
  sampling procedures and, 131–32
Oral drive
  adult personality and, 107–8
  development of, 1
  hunger and, 107
  persistence of in adult behavior, 108
  personality development and, 107

## P

Pain
  centrifugal force and, 74
  deep localization of, 74–75
  differences in, 75–76
  electric frequency and, 74–75
  electric stimulation and, 72
  mechanism of, 74
  multiple stimulation and, 73
  nerves and, 182–83
  nerve regeneration and, 76
  physiological disturbance and, 197
  stimulation of, methods of, 79
  stimulus diameter and, 71
  stretch of nerve terminals and, 71–72
  temperature and, 77
  thresholds of, 78
    neurosis and, 196–97
  *see also* Nerves; *and* Somesthesis
Paralysis agitans, nerves and, 184
Perception
  brain damage and, 176–77
  distortion of, counseling and, 271
  and judgment, studies on, 121–23
  motivation and, 122–23
  saliency of objects of, 121–22
Personality, 105–17
  action dynamics and, 110–15
  adolescent, development of, 238
  adult, oral drive and, 107–8
  attributes of, 95
  categories of, 105
  childhood experience on, 124
  culture and, 123–25
  displacement and, 110
  fantasy production and, 110–11
  gastrointestinal illness and, 198–99
  home control techniques on, 240
  level of aspiration and, 114–15
  maladjusted teachers on, 250

Personality (*cont.*)
maturation of, 97
measurement of, 94, 95–96
mobility and, 124–25
motivational systems and, 124
prejudice and, 117
projection and, 110–11
frustration on, 111
psychopathic, 192
rating procedures, 94–95
repression and, 111–14
semistarvation on, 194
small group, dimensions of, 141
structure, theory of, 115–17
tests of
application of physiological factors, 96–97
masculinity-feminity scales and, 260
triple, hypnoanalysis of, 194
*see also* Personality development; Repression; *and* Traits
Personality development, 105–9
cultural influences on, 109
family behavior and, 109
Freudian theory of, 105–6
infantile experience and, 106–7
oral drive and, 107
Personnel
armed services, 151–52
clerical, selection tests for, 157
forced choice method of rating of, 161
mechanical, 158
pilots, 157–58
flight performance criteria for, 161
psychology of, 151
rating of, 89
acquaintance and, 161–62
paired comparison method of, 162
Rorschach test and, 156
requirements, tests for, 153, 154
efficiency of, 154
sales and clerical, 156–57
salesmen, criteria of, 156
supervisory and professional, 155–56
important qualities of, 155
selection devices for, 155
teaching, 250–51
training of, 162–63
executives, 162–63
production during, 162
*see also* Industrial psychology
Phenyl ethyl alcohol, olfactory thresholds and, 80
Pitch
frequency and, 58
judgment of, 59
loudness and volume, 58–59
Prejudice
dimensions of, 140
education and, 246–47
effects of, 139
ethnic, 136–40
Prejudice (*cont.*)
ethnocentrism and, 117, 123
factors predisposing to, 136–37
intergroup contact and, 138
measurement of, methods for, 139–40
personality and, 117, 136–37
physical characteristics and, 137
projective play techniques and, 138
propaganda and, 138–39
reduction of, methods for, 137–39
scapegoat theory of, 137
Prestige, suggestion, imitation and, 121
Problem solving
abnormal fixations and, 299–300
bimodality and, 300
concepts, development of, 305–7
direction and, 297–98
discouragement and, 300
factor analysis and, 303
flexibility and, 302
frustration and, 300
individual differences in, 302–4
intuition and, 301
learning and, 297
practical judgment and, 303
previous experience on, 301–2
processes of, experimental analysis of, 297–302
reasoning and, 303, 304
set and, 298–99, 303
speed and, 304
symbolic processes and, 297–308
words and, 308
Projection
clinical methods and, 207
personality and, 110–11
Psychiatry
army personnel and, 190
nonmedical therapists and, 226–27
*see also* Behavior disorders; Psychodiagnostics; *and* Psychotherapy
Psychoanalysis
Blacky test and, 212
briefer methods of, 228
childhood problems and, 189
contemporary, 189
displacement and, 110
infant swaddling and, 109
oral development and, 1, 106–8
projection, 110–11
psychosexual development and, 200
repression, 111–14
systemization of, 105–6
Psychodiagnostics, 207–18
clinical study and, 215
clinicians and, 207, 217
objective tests in, 207–8, 213–17
problems of, 217–18
projective techniques in, 207, 208–13
*see also* Psychotherapy
Psychology, animal, *see* Animal psychology

Psychology, educational, 235–51
Psychology, industrial, 151–68
Psychology, physiological, 173–86
  brain damage and, 176–81
Psychology, social, *see* Social psychology
Psychometric methods, 285–87
  *see also* Psychophysical techniques; *and* Scale analysis
Psychopathic personality, 192
Psychophysical techniques, 285
  probit analysis, 285
  sensitivity data, analysis of, 285
Psychosis, of old age, 203
Psychosomatics, stress, anxiety, and schizophrenia, 185–86
Psychotherapy
  anthropological investigations and, 222
  brief methods of, 228
  case workers, reliability of, 225
  client talk and, 224
  education and, 238–39
  failures in, evaluation of, 225–26
  by general practitioners, 227
  group methods of, 228–29
  hypnosis and, 229–30
  interview analysis technique, 224
  nondirective, 223–24, 229
    evaluations of, 229
  nonmedical, 226–27
    by psychologists, 226
    by teachers and ministers, 226
    training for, 227
  other treatment methods, follow-up investigations of, 224–25
  perception processes and, 222
  personality changes and, 224
  physiological changes and, 224
  principles of, theory of, 221–23
  research in, 223–25
    awareness of need for, 223
  statistic reports and, 225
  techniques of
    new applications of, 228
    variations in, 227–28
  training for, 227
  trends in, 230
  unlearning and, 221–22
  *see also* Psychodiagnostics
Pulfrich phenomenon, visual functions and, 33–34
Punishment, on fixation, 193–94

## R

Radio serials, gratification and, 144–45
Reaction time, growth and, 2
Readiness, 236–37
  determination of, cultural, 236
  learning experience on, 243
  learning of, 237
  recognition of, 237
  social background and, 236–37
Reinforcement
  learning under satiated drive and, 18
  secondary
    conditions of, 19–20
    studies of, 18–19
  versus field-cognition, 15–20
Repression
  infantile amnesia and, 112
  interrupted task technique and, 113
  motivation on, 111–12
  stress and, 194
  task completion and, 112–13
  Zeigarnik effect and, 113
Research design, *see* Experimental Design
Retention
  electroconvulsive shock on, 26, 180
  experiments on, 27
  forgetting and, 26–27
  radio news and, 166
Rorschach test, 208
  applications of, 211
  basic score of, 209
  blind application of, 211
  counseling therapy and, 275
  criterion contamination and, 211–12
  diagnostic categories of, 209–10
  interpretation of, 209, 210
  levels of adjustment and, 211
  organic pathology and, 210–11
  psychiatric diagnosis and, 209
  suicidal configurations of, 210
Rumor, spread and distortion of, 144

## S

Scale analysis, 285–87
  Guttman's method, 285
  Likert's method, 285–86
  Loevinger's method, 285–86
  new methods of, 286–87
  qualitative variables, analysis of, 286
  Thurstone's method, 285–86
Schizophrenia
  adrenal cortical hormones and, 185–86
  classification of, 191
  environment on, 191–92
  fantasy and, 200–1
  frustration and, 198
  genetic determination of, 190
  language distortions and, 199
  pain threshold and, 197
  psychosomatics, stress, anxiety and, 185–86
  pursuit learning and, 193
  thinking in, 199
    normal adults and, 200
Sense organs, stimulation of, 53
Senses, chemical, 80–85
  somesthesis and, 71–85
Smell, sense of, 80–83
  acuity of, 81–82
  anosmia and, 82

Smell, sense of (*cont.*)
appetite and, 82
chemical sensitivity of, 83
experimental apparatus and, 81
hallucinations and, 202
physiology of, 81
thresholds of, 82–83
differential, 80
Social psychology
action research and, 120
applied research on, 120
armed services and, 152
basic concepts of, 119
cognitive processes in, 121–23
group processes and, 119–47
opinion research and, 120
personality and, 119–20
trends in, 140
UNESCO and, 145–46
warfare and, 146–47
world affairs and, 145–47
Social status
adolescent patterns and, 128
children's concepts of, 237–38
income satisfaction and, 129
on learning, 237
psychological consequences of, 127–28
social experience and, 237–38
Somesthesis
abnormal sensitivities and, 79
basic mechanisms of, 71–75
chemical senses and, 71–85
clinical techniques and, 77–79
experimental apparatus and, 79–80
extinction and, 77–78
mechanisms of, 75
neural correlates of, 75–77
*see also* Pain
Sound
deleterious effects of, 64, 168
localization of, 63–64
bodily rotation and, 63
precedence effect and, 63–64
subtraction of, 58
Speech
aphasia, delayed, 181
of children, 247
development of in infants, 1
distorted, 62–63
hearing of, 62–63
intelligibility of
altitude on, 166
distortion and, 62–63
stimulation of, 181
local anesthesia and, 181
paralyzing effect of, 181–82
theory of, 182
Stanford-Binet Test, levels of adjustment and, 211
Statistical methods
coding, addend method of, 291
Statistical methods (*cont.*)
computational techniques, 291–92
*F*-test, two tailed, 283
nonparametric tests, transformations and, 283–84
power functions of statistical tests, 284
sampling, 284–85
area versus quota, 284
*see also* Chi-square; Correlation; Experimental design; Factor analysis; Multivariate tests; Psychophysical techniques; Scale analysis; *and* Variance, analysis of
Statistical theory, research design and, 277–93
Stimulation, methods of, 181–83
Stress, 195–97
battle, 195–96
psychosomatics, anxiety, and schizophrenia, 185–86
repression and, 194
Strychnine, on speech, 181–82
Suggestibility, neurosis and, 202
Suggestion, imitation, and prestige, 121
Suicide, Rorschach test and, 210
Suppressor systems
cortical activation pattern and, 183–84
functions of and thought, 183–85
spasticity and, 184
stimulation of, 184
waking center and, 184–85
Symbolic processes, problem solving and, 297–308
Syntality, of small groups, dimensions of, 141

## T

Taste
ageusia and, 84–85
alkalinity and, 1
relative intensities and, 83–84
scales of, 83
Temperament, factorial analysis of, 97
Terman group test, Lima Revision of, 4
Tests, aptitude, *see* Aptitude tests
Tests, interest, *see* Interests, tests of
Tests, objective, 213–17
abbreviated screening tests, 215
diagnostic effectiveness of, 214–15
profile standards and, 216
psychiatric screening and, 214
psychometric scatter patterns, 215–16
status of, 213–14
variations of, 217
Wechsler-Bellevue test, 216
Tests, projective
drawing of human figure as, 212
handwriting as, 212
Rorschach test, 208
situations test, 213

Tests, projective (*cont.*)
Szondi test, 212
Thematic Apperception Test, 208
variations of, 212–13
Thematic Apperception Test, 208
levels of adjustment and, 211
response analysis and, 262–63
Thiamine, on intellectual development, 99–100
Thought
analysis of, 185
language and, 199–200
schizophrenia and, 199
suppressor functions and, 183–85
Threshold
of frustration, 197
of hearing
absolute, 49–51
energetics of, 52–53
long time, 51
short tone, 51–52
of pain, 72–73, 78
neurosis and, 196–97
of smell, 80, 82–83
of taste, 83
of vision
depth, 33
monocular movement parallax and, 33
quantal variations on, 42–43
Tone
volume of, loudness, pitch, and, 58–59
short bursts of noise and, 53–58
Traits
congruences of, interrupted task technique and, 113–14
individual, factor analysis of, 90–92
nature and organization of, 90–97
syndromes of, 117

## U

UNESCO, social psychology and, 145–46

## V

Values, opinion and ideology, determinants of, 127–30
Variance, analysis of, 278–81
assumptions in, 280
extreme deviate, interpretation of, 280
homogeneity of variance, absence of, 281
independent variable on, lack of control of, 281
interaction, sampling variation, 280
interpretation of, 280
nonorthogonal designs and, 280–81
*see also* Experimental design
Veterans, college and, 102
Vision, 31–43
acuity testing, 40–41
empiricism of, 41
statistical techniques and, 41
brain damage and, 177
brightness discriminations and, 31
color testing, 41–42
hallucinations and, 201
methods and equipment for study of, 31–32
phosphor screens, 32
recording and measurement aids, 32
short flash generators, 31–32
testing of
advances in, 40–42
pilots and, 157
*see also* Visual functions
Visual functions
acuity and, 34
aftereffects
receding patterns and, 35
satiational theory of, 35–36
angle interpolation and, 38–39
cerebral damage and, 42
color theory and, 31, 39–40
chromatic adaptation, 39
depth perception and, 32–33
geometry of binocular vision, 32
Howard-Dolman test of, 33
depth thresholds and, 33
distortions
form and shape, 35–36
intensity, 35
tilt, 35
electroretinograms and, 42
experimental equipment and, 37
experimental test techniques and, 37
figural aftereffects and, 35
Hunter-Sigler effect and, 34–35
light and dark adaptation, 40
luminosity and, 39–40
monocular movement parallax thresholds and, 33
nerve impulses and, 42
ocular tremor and, 34
perception of the vertical, determinants of, 36
physiological bases of, 42–43
psychophysics, social psychology and, 36
Pulfrich phenomenon and, 33–34
reading efficiency and, 36–38
techniques of measurement of, 37–38
research on, 32–40
span and, 34–35
successive flashes and, 40
thresholds, quantal variations on, 42–43
visual purple, 42
*see also* Vision
Visual purple, physiological bases of, 42

Vocational guidance, 248–49
  value of, 249
  *see also* Counseling methods
Voting behavior, 134–36
  shifting of trends and, 135–36
Voting polls
  capabilities of, 134
  failure of, 134
Voting polls (*cont.*)
  forecasting errors in, 135
  sampling errors in, 135

## W

Warfare, psychological, 146–47
Words, problem solving and, 308